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OBITUARY NOTICE

SIGMUND FREUD, 1856–1939

WITH the death of Sigmund Freud on 23 September 1939, at the age of 83, one of the great figures of psychology has passed away—we can truly add, one of the great figures of our own time, for Freud has undoubtedly exercised a wider influence upon his generation than any other psychologist. He effectually cleared psychology from the charge of being academic, of being (as H. G. Wells, one of Freud's personal friends in his last days, once put it) "a happy refuge for the lazy industry of pedants". Freud's work made it clear that psychology really had something of importance to say of human life as actually lived from day to day, of men's problems and worries, of their loves and hates, their heroisms and their follies, of the heights and depths of their innermost aspirations and desires. If much of his message was unwelcome, it was nevertheless vivid and compelling, and altogether free of that quality of abstraction and remoteness from ordinary life that seemed to the plain man to be characteristic alike of the philosopher's study and the experimentalist's laboratory: and hence it is that the name of Freud is to-day almost certainly better known throughout the world than that of any other worker, past or present, in the field of mental science.

As a thinker and observer, Freud was at once intensely original, immensely assiduous, courageous and self-confident in a way that was compatible with calm and penetrating self-criticism, and endowed with an astonishing breadth of vision that enabled him to realize the full implications of his own discoveries.

His originality was shown not only in the multitude of instances in which he brought new insight to the elucidation of the obscurer forms of human thought and motivation, but also in his whole attitude towards the problems of psychology. Although many before him had found a place for the "unconscious", "subconscious", or some similar concept, in their systems, either on general philosophical grounds or as needed for the explanation of certain unusual or pathological phenomena, Freud was the first to make such a concept the really essential element in a psychology far-reaching in its scope but fundamentally empirical in character, built up as it was on the basis of his own painstaking observations over many years. His use of original intuition, both for seizing on significant facts

and for viewing them in meaningful perspective, has certainly never been surpassed, and seldom if ever approached, in the whole history of psychology. In this respect, however, he was, in his own exceptional degree, true to the great German tradition, which (as has been well brought out in recent discussions in this country) has always attached importance to happy insight and ingenious speculation and has been little attracted by the mere accumulation of facts and measurements for their own sake which has sometimes found favour in America. Facts there are of course in plenty in Freud's writings, but they have mostly been used, almost as soon as obtained, for the purpose of some suggestive addition to, or reorientation of, psychological theory.

Freud's assiduity is manifested eloquently in the great number of his books and papers. The list of his writings up to 1932 occupies 14 pages of Murchison's *Psychological Register*, and (excluding translations) contains 225 items, of which some 20 represent the predominantly neurological interests of his pre-psychoanalytic period. His *Gesammelte Schriften* in their forthcoming final edition will run to sixteen imposing volumes. When we remember the pregnant and condensed nature of many of these contributions and also the fact that they were written by one whose working hours were largely occupied with a novel and very time-consuming form of clinical practice, we cannot but be impressed by the mere quantity of his literary output, quite apart from its value and originality.

What we have ventured to call Freud's courage and self-confidence are revealed in many aspects of his life and work. The fact that he was able to persist for many years in his chosen line of investigation in spite of disapproval or neglect from scientific colleagues, and to contemplate with equanimity the likelihood that his work would never be appreciated in his own lifetime, finds of course a parallel in the career of many others who have made signal contributions to knowledge (and indeed it may be said that Freud had the consolation, denied to them, of knowing something of the reason why his own work encountered rebuff or incredulity). More specially characteristic of him, however, was his ability to trust to the path of psychology and not desert it even when it appeared to have misled him. The classical instance of this occurred on his discovery that many of the accounts of sexual seduction in early years which he heard from his patients were no more than figments of these patients' imaginations. Instead of being led to a fundamental scepticism concerning the value of what his patients told him, and perhaps to an equally fundamental distrust of his own judgement, he realized that what was here

important for his own purpose was, not so much the distortion of objective fact as the widespread existence of the phantasy in question—the psychological reality rather than the physical one. His belief in psychic determinism, which had been throughout one of the essential postulates of his thinking and had led him to make such extensive use of the concept of the "unconscious", here stood him in good stead, and allowed him to see what most others would assuredly have missed. In this and other matters, however, he did not hesitate to acknowledge and correct his own errors as soon as they became apparent to him—at whatever cost to his theoretical assumptions (as when he felt himself compelled to adopt the concept of Narcissism, thus enlarging still further the immense and by no means popular role he had assigned to the *libido*, and obliterating the neat theoretical distinction between this and the ego-trends), or to his reputation and esteem (as when in later years he expressed astonishment that he could so long have neglected the role of aggression in human life). But in recognizing the limitations of his earlier views, he was careful to avoid losing touch with the solid ground he had already won. Indeed, he had a very clear realization of the importance of his own work, and it was he himself who instituted the comparison (with regard to their effect on human complacency) between his discoveries and those of Copernicus and Darwin—a comparison which hostile critics did not omit to stigmatize as a sign of his immodesty.

Most of all perhaps was his courage shown in his resolute facing of the less agreeable aspects of reality, whether in his own patients, in human disposition generally, in the limitations of our psychological knowledge and our therapeutic power, or in the very nature of life itself and its bearing upon human destiny. Certainly the author of *The Future of an Illusion* and *Civilization and its Discontents* was himself free (as many felt, painfully and embarrassingly free) from any illusion as to the existence of Utopia, whether in the past, in the present, in another world, or in an age to come. It seems likely indeed that this aspect of his character was an essential condition of his making the discoveries he did, of his being able to penetrate the veil which, on his own showing, protects us from the disturbing knowledge of our unconscious tendencies. But Freud differed also from the ordinary pessimist or breaker of unwelcome news, in that he availed himself of none of the compensations to which the latter is wont to resort. He appears to have derived but little satisfaction from the shocks he gave to men's respectability and equanimity. This manifestation of sadism, one which is assuredly a temptation to the psycho-analyst, as to all who have incurred moral ostracism or broken cherished illusions,

he seems to have held in remarkable control, when we bear in mind the unique opportunities he possessed in this direction. Indeed the two essays written in reference to the Great War (which can profitably be re-read at the present time) evidence a certain tender yet intensely virile sympathy, which is entirely free, alike from the usual wartime bias and enthusiasms, and from any vulgar self-justification in the spirit of "I told you so" in which a lesser mind might have been tempted to indulge. He never became bitter or cynical, nor did he (as some distinguished pessimists have done) utter mocking taunts to man upon his folly or defiant protests to the universe on its indifference or its cruelty. There is something which can only be called heroic in Freud's attitude towards suffering and evil, an attitude without rancour, lamentation or complaint. One is tempted to regard him, the bringer of great gifts to mankind, as a modern Prometheus—a noble figure such as the Greek tragedians would have lovingly depicted. But perhaps it is better to note the impression that this aspect of his thought and character has made upon an eminent contemporary. Speaking of the "pessimistic philosophy of life" set forth in *Beyond the Pleasure Principle*, Sir Percy Nunn says that he there "outlines a conception so tragically grand that it extorts admiration, yet so terrible that it seems to make even his most faithful followers uneasy".[1]

A fourth element of great importance in the development of Freud's thought was his breadth of outlook and capacity for seeing resemblances in matters which had hitherto been regarded as remote and unconnected. It is principally owing to this characteristic that psycho-analysis started as a therapeutic method for dealing with neurosis and ended by becoming a discipline that has affected almost every branch of knowledge that deals with the life and works of man. The general history of psycho-analysis is too well known to need repeating here. It is not necessary to remind readers of this Journal how Freud was able to apply what he had learnt in his consulting room to dreams (which he himself always regarded as one of the most important and assured of all his conquests), to what he called the "psycho-pathology of everyday life", to child psychology (another vastly important field), to wit, humour, art, literature, history, social psychology and anthropology. We will confine ourselves to recalling two examples of this ability to detect unsuspected resemblances. The first is the parallel he drew between the "pre-genital" development of the young child and the behaviour of the adult sexual "pervert", a comparison which underlies the epoch-making *Three Contributions to the Theory of Sex*, itself the basis of much of that great increase of knowledge

[1] In the Preface to Barbara Low's *The Unconscious in Action*, 1928.

in the sexual sphere that we owe to psycho-analysis. The second example consists in the remarkable series of resemblances he pointed out between the neurotic mind, the child mind and the mind of primitive man (his much discussed theory of totemism will serve as a specific instance). Before the appearance of *Totem and Taboo*, abnormal psychology, child psychology and cultural anthropology had been distinct disciplines with few if any points of contact. By bringing them into relation with one another, Freud fructified them all, and laid the foundation of a new comparative psychology of the human mind, the full consequences of which are only now beginning to appear, with the advent of a new generation of workers (as yet unfortunately but few in number) with sufficient training to apply the knowledge gained in adjacent fields to the special problems of their own.

To the present writer it has always seemed that Freud's scientific work in the field of psycho-analysis may conveniently be considered as falling into two main periods. In the first, extending from his collaboration with Breuer till shortly after the end of the war, he was mainly concerned with the fundamental mechanisms of the unconscious and the protean manifestations of the *libido*. It is this part of his work which, up to the present, has received the wider public recognition and has more largely influenced current thought. On the other hand it was with regard to this period also that the charge of "pan-sexualism" has most often and most plausibly been brought against him. Freud, however, was himself all the time conscious of the fact (which he affirmed repeatedly, but not often enough to satisfy his critics) that there were other important aspects of human personality which had as yet scarcely been touched by psycho-analysis.

In *Beyond the Pleasure Principle*, which appeared in 1920, he is seen dramatically groping his way towards the immense problems presented by this larger field, a field that can be regarded as definitely entered with the publication of *The Ego and the Id* in 1923. From that time onwards he was largely occupied with the repressing—as distinct from the repressed—factors, with aggression and with the growth and nature of human morality; which latter, in his hands, soon revealed itself as resulting from the interplay of a highly complex set of forces, many of them obscure and sinister in their origin and implications. Although as yet less generally appreciated, there can be little doubt that this later work has an importance at least equal to his earlier formulations with regard to sex. Freud has shown that man's individual difficulties and his social failures are due, not merely, as moralists throughout the ages

seem usually to have thought, to inadequate control over strong a-social impulses, but also to the primitive, chaotic and archaic nature of much of his morality itself, which is often no less unreasonable and unrealistic than the clamourings of instinct, and which is moreover liable to be highly charged with that fatal aggressiveness which, as many are beginning to think, may lead to the downfall of our present culture.

Great as were his adventures in the realm of thought and observation, Freud's external life was for the most part calm and uneventful. Born at Freiberg, Moravia, on 6 May 1856, he lived in Vienna from the age of four till about a year and a half before his death. He attended the University from 1875, obtaining the degree of Dr.med. in 1881. He was Dozent for Neuropathology from 1885 to 1902 and became Professor Extraordinarius in the latter year. It was only in 1920, when he was already world-famous, that he received from his own University the full academic recognition implied by the title of Professor Ordinarius (though actually he never held the position).

In 1885 he studied in Paris with Charcot. In general those who know him best are inclined to think that the influence that the great French psychiatrist exercised on Freud was not decisive. In one matter at least, however, Freud himself suggests that Charcot's words were important for the subsequent development of his own thought (though in this connexion he links Charcot's name with those of Breuer and Chrobak). Speaking of certain symptoms presented by one of his cases, Charcot once remarked privately: "Mais, dans des cas pareils, c'est toujours la chose génitale, toujours—toujours—toujours", and at the same time emphasized his words by jumping up and down "with the vivacity peculiar to him". Freud wondered at the time, why, if Charcot held this view so strongly, he did not mention it in his official lectures. Herein perhaps we may detect a significant difference between the two men, one in which the peculiar courage of Freud, for whom truth was always more important than discretion, surely played its part.

Shortly after his return from Paris he renewed his friendship with Josef Breuer—to whom he already owed some inspiration and who had already evolved the "cathartic" treatment which can be regarded as the fore-runner of psycho-analysis—and began the collaboration that eventually led him, when continuing the work alone, to the development of the psycho-analytic method and all that followed from it. Their joint work, *Studies in Hysteria*, was published in 1895 (though some of the constituent papers had appeared earlier), and this event is usually reckoned the first in the history of psycho-analysis.

From 1902 onwards Freud ceased to work in isolation. A few others, convinced of the truth and importance of his contentions, began to gather round him. The little group enlarged slowly, and in 1908 there was held the first meeting of what was to become the International Psycho-analytical Assocation, Ernest Jones being the earliest and most prominent British representative. This Association (with constituent societies in various countries) has, since its foundation, officially controlled the destinies of the psycho-analytic movement.

In 1909 Freud visited America at the invitation of Stanley Hall and delivered at Clark University the subsequently published *Five Lectures on Psycho-analysis*, receiving at the same time the degree of LL.D of that University. Apart from this he seldom left his native land except for attendances at the congresses of the International Psycho-analytical Association and for fairly frequent visits to Rome, whither he was strongly attracted by his archaeological interests. In 1938 however, on the Nazi occupation of Austria, he was compelled to leave Vienna, the city where he had lived since his early childhood, and the last year and a half of his life were spent in London, where he received a welcome which touched him very deeply. Some time before his arrival he had been made a Foreign Member of the Royal Society; while in 1926 he had accepted Honorary Membership of the British Psychological Society. It was his hope, had he lived longer, to become a British citizen.

In his two writings of an autobiographical nature Freud has told us much about the development of his ideas and the early history of psycho-analysis, but little concerning his own intimate life or his personal feelings on matters unconnected with his work. Where he himself has been so reticent, it would be unseemly at the present moment to try to pierce the veil which he so clearly wished to be drawn over his purely personal affairs. There can be little doubt however that future psychologists and historians of science will display a great and legitimate curiosity as to what manner of man it was who was able to penetrate so much further than others into the depths of the human mind. Those who were privileged to meet him realized at once that he possessed not only a great intellect but a great personality. Of many remarkable characteristics perhaps the most striking was his sincerity—charming, but at the same time (accustomed as we are to more tortuous methods of social intercourse than he employed) startling and disconcerting. He once wrote that one of the inconvenient consequences of his long preoccupation with psycho-analysis was that he found it impossible to tell a lie. When face to face with him one realized that this statement was less outrageous than it

might at first appear; one felt inclined to believe also that its converse might also contain an important element of truth, and that it was largely because of his courageous (and inevitably disconcerting) frankness in thought and word that he had been able to become the founder of psycho-analysis.

Freud was a delightful speaker and lecturer. Disdaining the arts of the orator, he yet gripped his audience with his first words, and created a peculiar atmosphere of intimacy in which the hearers felt themselves, as it were, to have been granted the privilege to listen-in to the reflexions of a remarkable mind. The same charm of intimate personal contact pervades many of his writings, both those in which we get the impression that we are watching the master in the very process of wrestling with new and difficult ideas and those admirable didactic expositions in which he himself was on familiar ground and only concerned with making the path smooth for those with lesser knowledge. Added to this, his later works are suffused with that quality of strong yet reposeful mellowness and detachment from the clamorous urgencies of life, that comes characteristically to certain of the greatest minds in their full maturity.

On the astonishing nature and far-reaching import of Freud's contributions to science, psychologists are all agreed. Like others, they have in their time been startled and shocked at many of Freud's contentions, and this bewilderment has sometimes found rationalized expression in the form of scientific or quasi-scientific objections. The day of such rationalizations is approaching its end, but there remain some real and difficult problems with which psycho-analysis still confronts psychologists (and among psychologists we here include psycho-analysts themselves). There are two such problems of a general order, though perhaps they can be more accurately looked upon as two aspects of a single problem. In the first place, how far and in what ways can psycho-analytic discoveries be reconciled with the findings of psychologists of other schools who have worked with other methods? And in the second place, how far do these discoveries admit of rigid proof by the accepted canons of scientific evidence (for it can scarcely be denied that psycho-analysis does in fact raise some knotty questions from the point of view of logic and scientific method)? The two problems tend to be one, inasmuch as other schools are often in possession of more unquestionably valid scientific methods, whereas psycho-analysis often presents us with conclusions which, if true, are clearly of greater practical and theoretic value. (As the matter has sometimes been put, vividly but with evident exaggeration, by those who are sceptical of all psychology, what has been proved by psychologists

was not worth proving, while what is important has not yet been proved). The problems have arisen in this form partly no doubt because of the divorce that has always existed and to a considerable extent still exists (though it is less in this country than in some others) between psycho-analysis and every variety of "academic" psychology. A separate origin may have been inevitable; indeed it has often and very plausibly been said that psycho-analysis could never have been invented by anyone brought up, say, in the Wundtian tradition. But a permanent separation is unthinkable, and the sooner and more efficiently the gulf is bridged, the better it will be for psychology as a whole. The process of bridge-building is difficult and likely to tax the ingenuity of psychologists for years to come. Fortunately the work has already begun, and from both sides, though in a humble way; certain psycho-analysts, for instance (notably perhaps Franz Alexander and his colleagues in Chicago), are beginning to pay attention to the planning of controlled and systematic investigations of particular problems, while experimental psychologists on their part are finding that some at least of the Freudian doctrines can be put to the test by adaptations of existing experimental techniques and can in turn help to illumine the data obtained by such techniques.

What modifications Freudian theory may undergo as the result of its fuller incorporation into general psychological thought, it is impossible to tell at present. As a theorist, Freud himself was never over-much concerned about the impregnability or permanence of his theories. He used them, as theories should be used, to bring about a temporary clarification of his thought, confronted as it was by his strange discoveries, and as stepping stones to his further advance. Indeed we receive the impression that he never surveyed his theoretical structure as a whole. This he left for others to do; and, as we know, he would unhesitatingly scrap any item of theory that no longer served its purpose. We know too that such process of scrapping left his essential findings for the most part undisturbed. These findings have been, and doubtless will again be, revealed as incomplete, but none have as yet been shown to be fundamentally unsound or illusory. On the contrary, their value has been proved by the very fact that they have so often led to new and wider findings. Freud himself, though his life was devoted to the investigation of the less reasonable aspects of the human mind, was a firm believer in the ultimate value and power of the still small voice of reason; and the greatest service that psychologists can render to the memory of this great thinker is to use his contributions to their science for further and fuller investigations in the light of reason. Had Freud been granted a

longer life in this country, to which he came so late but whose welcome he so much appreciated, he would doubtless have encountered, and found himself thoroughly in sympathy with, the noble motto of one of our most ancient university towns—one which has itself lately accorded a welcome to psychology: *magna est veritas et praevalebit.* It is as a relentless and unremitting searcher for truth in the most difficult of all places, the secrets that men are unable and unwilling to disclose even to themselves, that Freud's reputation is secure, now and for all time.

J. C. Flugel.

THE PROBLEM OF THE DIRECTION OF THOUGHT

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I. Introduction: the orderly succession of thoughts

When one examines an actual train of thought as it occurs in a human being two primary and distinct problems seem to emerge. There is first of all the problem of what may be called the material of thought. Of what is the train of thought composed? Does it consist of ideas, of images, of imageless thought processes, of Gestalten, or what not? Or, if one prefers to speak in terms of behaviour, should the most highly adaptive human activity be analysed into conditioned reflexes, behavioural Gestalten or into what unit, if any? On the other hand, there is the problem of why one member of the train succeeds another, why a particular idea or image occurs, more or less relevantly to the point at issue, instead of the million and one irrelevancies that might occur. Objectively speaking, in a train of the most highly adaptive behaviour what causes one activity to follow another relevantly to the situation? This is the problem of mechanism. It was Aristotle who first called attention to the fact of the orderly sequence of mental processes. "Recollection occurs", he says, "because processes naturally follow each other in an orderly manner" (1). It is primarily to the problem of the orderly sequence of thinking that this paper will be devoted, although it will appear that the two problems cannot ultimately be separated. In order to discuss mechanism intelligently it is necessary to a certain extent to consider material and vice versa.

II. Associative hypotheses

(1) *Early theories*

On what principle does the orderly succession of thoughts take place? Undoubtedly the most influential hypothesis has been that of association in some form or other. Suggested by Plato, elaborated by Aristotle, annotated through the Middle Ages, the doctrine of association gave its name to the dynasty of philosophers and psychologists known as the British Associationists. From such philosophers as Hobbes, Locke and Hume the psychologists characteristically took over the doctrine, and we find a succession of names such as the Mills, father and son, Bain, Spencer and Lewes, forming a group who may be said to comprise a pre-experimental school of associational psychologists. Thus it came about that the advent of experimental psychology found ready made a hypothesis for the mechanism of thinking. And thus it was, also, that, although there were important dissentients, the associational hypothesis may fairly be said to have been the conventional explanation of the sequence of thoughts when the Würzburg group began to work on the psychology of thinking at the beginning of the present century.

The main hypothesis of the orthodox associational psychology was of course that all other phenomena of consciousness are elaborated from the elementary sensations by means of association ((2), p. 259), association being roughly defined as the process by which, if two experiences have occurred together, the one will tend to reproduce the other at a future time. Two separate hypotheses are really involved; that which sets up the sensation as the sole primary datum and that which sets up association as the sole elaborating mechanism. The discussion of the primacy of the sensation belongs under the heading of the material of thinking, and is thus not specifically relevant to this paper. Two remarks are, however, in order. The first concerns the impressive name of Aristotle. His well-known dictum that there is no thought without image has been quoted in support of the sensationalist argument. For example, Titchener ((3), p. 117) classes Aristotle as a sensationalist. However, the course of Aristotle's argument makes it fairly clear that in this passage the philosopher means exactly what he says and no more. Anybody who reads without prejudice the relevant passages of the *de Anima* may convince himself that Aristotle distinguishes thinking from the images which are utilized in thinking.[1]

[1] See, for example, *De Anima*, 427*b*: "Imagination (i.e. the formation of images) is different from either perceiving or discursive thinking, though it is not found without sensation, or judgment without it" (trans. Smith). Similarly our concepts "are not images though they necessarily involve them" (*ibid.* 432*a*).

Not that psychology of to-day has to rely on Aristotle to tell us what is the nature of thinking. But if the authority of the name is to be invoked at all, this should be done not in support of the sensationalist hypothesis, that thinking is the elaboration and manipulation of sensation, but rather of the very different one that thinking of a non-sensory nature utilizes experience of a sensory nature. This latter hypothesis cannot properly be classed as sensationalistic.

The other remark concerns the question of analysis. In order to account for actual experience the later associationists had to postulate that association not only binds experiences together, but also transforms them. Thus, for example, we have the younger Mill's 'mental chemistry', by which associated sensations are mutually modified and which substitutes the chemical analogy of the union of molecules for the mechanical one of Mill the elder. Now this is a perfectly legitimate assumption, but it does depart from the original spirit of the associational hypothesis according to which one experience reproduces another. Stout pointed this out forty years ago. In fact, the stricter associational doctrine current at the beginning of the experimental period reverts to the doctrine of reproduction at least by implication. For the phrases, viz. 'associative recall', 'reproductive tendency', and so on, current at the time the Würzburg School began their work thirty-five years ago, do imply that the original experience is reinstated, not remade. The associative psychology, as originally understood, and as adopted by the early experimentalists, is essentially a psychology of the reinstatement of past experience. But we have seen that a psychology of reinstatement had previously been found to be inadequate. That is to say, it was found impossible to account for what actually happens during thinking by the sole assumption that any process of experience has the tendency under specified conditions to reproduce another with which it has been associated.

At the end of his history of the Association psychology Warren sums up the difficulties as follows. "It would appear that the associationists included under the term *association* two or three rather distinct operations.... To group these three operations under a single name", he continues, "is a verbal simplification scarcely justified by the facts with which we are dealing.... Again", he further continues, "the phenomena of *attention* and *discrimination* do not seem to be accounted for under the associationist's treatment. These phenomena appear to involve distinctive operations upon the elementary data. In other words, instead of accounting for all complications of experience by means of one single type of operation, we seem justified in assuming several different operations" ((2), pp. 306–7).

(2) *Thorndike's associationism*

In spite of these difficulties to which attention has been called at many different times it is fair to say that the associational theory is very much alive to-day as part of the background of many able psychologists. Its vitality is shown by the fact that it has survived translation into behaviouristic coordinates. Thus Thorndike's explanation of thinking as due to the operation of bonds shows leanings towards behaviourism. The bonds in terms of which he casts his whole system are ultimately associational. It may here be noticed that, perhaps by historical accident, what may be called objective associationism has taken into account a problem not sufficiently recognized by its subjective counterpart. I refer to the problem of the rejection of error. So much impressed, indeed, were the early comparative psychologists with the fact of the elimination of mistakes that they exalted 'trial and error' into a religion, some indeed still maintaining that it provides a psychological criterion between man and the animals, others that human thought is of an essentially trial and error nature, though on a plane not reached by animals. Here it is hardly necessary to mention that trial and error cannot provide an explanation of the direction of thought, whether animal or human, or whether the discussion is conducted in subjective or objective terms. Trial and error is a matter of observation, something to be explained rather than a principle of explanation. In the total picture, the fact that irrelevant processes are rejected is the counterpart of the fact that more or less relevant processes are initiated. The early animal workers performed a great service by throwing into relief the corrective aspect of directed behaviour; but they did not solve the problem of why one process succeeds another. They rather added or perhaps stressed a complementary set of data, often, in their enthusiasm, losing their vision of the whole picture in a contemplation of that part of it which their own methods had thrown into prominence.

(3) *The conditioned reflex theory*

Another, more stringently objective, type of associationism must now be considered, viz. the well-known conditioned reflex theory. Like their predecessors, the classical associationists, the conditioned reflex enthusiasts claim to be able to analyse the highest human activity into elementary data—the conditioned reflexes—originating in the stimulation of the sense organs, such data, like those of the subjective theory, being elaborated into more complex wholes by means of a single mechanism,

that of the process of conditioning. Thus for each of these two associational hypotheses the highest activity, of thought and action respectively, differs only in complexity from the process ensuing upon elementary sense excitation. It will be seen that in his effort to avoid what he considered to be the mysticism of the subjective psychology, Watson succeeded only in repeating the programme of perhaps the most reactionary of the subjective schools. Instead of solving the problem he had merely transformed its coordinates.

Like its predecessor, the conditioned reflex theory is essentially one of reproduction of the past. The conditioned stimulus in Watson's and Pavlov's theory, at least in its earlier form, is attached to, and later reproduces, the response which originally belonged to the unconditioned stimulus. It is interesting to find that the exact measurement made possible by objective registration supplements the criticism already made of the subjective associational doctrine. The earlier associationists had found that a doctrine of reproduction was inadequate to explain the facts. Objective registration demonstrates that at least in the conditioned reflex, which is the point at issue, reproduction does not take place. Examination of the records, both those of Pavlov and of other investigators, show that, in the overwhelming majority of cases, the conditioned reflex is quantitatively different from the unconditioned one, being generally very much the smaller of the two; there is also some evidence that, in certain cases, it is qualitatively different. Hilgard has brought forward convincing proof that at least in the case investigated by him it might occur in addition to the unconditioned reflex, as a supernumerary movement, an entirely new response, which could not possibly be a substitute or a reproduction (4). Whenever, in fact, the hypothesis of sheer reproduction or reproductive combination has been used in the description of the highest psychological activities it has been found inadequate. On the one side, it is found not to account for the observed facts; on the other, it postulates a process on the existence of which careful examination tends to throw doubt. On the side of subjective psychology reproduction of the past is denied, as a working mechanism, by workers of such widely different interests as F. H. Bradley (5), F. C. Bartlett (6), Stout (6a) and the Gestalt group, of course in different contexts. And in the same way we find such objective psychologists as are best qualified to speak—as Lashley (7) and Hull (8), for example—rejecting the analysis of all behaviour into reproductive conditioned reflexes, and maintaining that the problem of the analysis of action is not yet solved. For much the same reason, then, that the classical association must be rejected as an explanation of

the directed or orderly flow of thought as experienced, so must the conditioned reflex be rejected as an explanation of the orderly progress of the most complex action.

(4) *The Würzburg school*

It is well known that, at the beginning of the century, Watt(10) and Ach(11), of the Würzburg group, directed experimental attacks on the problem of the orderly flow of thinking, probably acting on a hint which Külpe dropped in 1893. They produced very similar solutions though they used different terminologies. Associationism was retained as the basic mechanism of thinking; but it was supplemented by another factor, which Watt calls the *Aufgabe* or task and Ach the 'determining tendency'. According to Ach, for example, the determining tendencies facilitated relevant reproductive tendencies and inhibited irrelevant ones. When, in a multiplication sum, I meet the figures 6 and 5, I do not put down 11, $1\frac{1}{5}$, or 1, but 30. These false associations are ruled out, and the correct one facilitated, according to the theory, by the determining tendencies. The working of the determining tendencies answered the question which had apparently puzzled the Würzburg group, as to why, of all the associations clustering round a given presentation, to use their language, the *relevant* association should appear during the process of thinking. But their whole scheme was still based on the doctrine of reproduction. One becomes tired, in fact, when reading their papers, of the continued iteration of the phrase 'reproductive tendencies'. Particularly is this true of Ach's paper, who weaves a nexus of reproductive tendencies and determining tendencies reminiscent of the 'properties' attributed to objects by the mediaevalists. Because it is thus founded on the theory of reproduction the Würzburg solution is subject to the same criticism. This should be made explicit. The classical association theory claims that if two experiences occur together, and one of them later reoccurs, the other is reinstated. Watt and Ach claim that out of many candidates for reinstatement the relevant one is selected by means of some extra-associational factor. But it has been seen that the hypothesis of reinstatement is untenable. A further difficulty of the Würzburg theory will be mentioned later.

III. Non-associative theories

(1) *Dynamic tendencies*

It might seem possible so to amend the doctrine of association as to replace the action of reproduction by one of representation. A given experience would then be followed by another in which a previously

associated experience would be represented, though not reproduced. The *Aufgabe* or determining tendencies would render the process specific. Reproduction, such as seems to occur in the nonsense syllable experiments, would be a limiting case imposed by special conditions. This formula which is really the one proposed by John Stuart Mill might seem to provide a possible mechanism for the orderly process of thinking, and without contradicting experimental fact. Such a scheme, however, would not be so much an emendation as an abandonment of the associational hypothesis. For, as many workers have pointed out, including Prof. Warren in the passage quoted, as soon as the notion of reproduction is abandoned, we must assume positive forces at work making for change, active, non-associational principles by which the past is represented in the present. This alone gives an entirely different aspect to the whole picture. We now have the task of describing the nature and mode of action of such forces. Further, supposing the existence of such hypothetical forces had been demonstrated, we would not then be entitled to assume that the experiments of Watt and Ach, which were performed under the limiting case when they were negligible, could validly be used to draw conclusions concerning those processes of ordinary thinking where they were operative. In other words, to eliminate reproduction from the picture is not a negative, subtractive, change, in the hypothesis; it really involves the introduction of positive factors which would invalidate the experimental evidence. And apart from reproduction there are other serious difficulties.

Lewin's well-known experiments(9) have cast some doubt on the ability of one experience as such to cause another to arise because the two have occurred together in the past, whether the process is one of reproduction or of any other kind. The couplings or bonds between mental events are not, he claims, the motors of future mental events. That is to say, for Lewin the energizing force which causes one event in a train of thought to follow another is rather the total play of mental forces operative at the present moment, which uses past experience as purely inert material. Connexions between events are important but only as determining the form which mental events will take, not as initiating them. This point is misunderstood in Thorndike's criticism.

We are here brought up against one of the chief problems in the modern psychology of thinking. What is the relation of present to past experience in the mechanism by which one process follows another during thought? Does the past effect a dynamic intrusion on the present, in that bonds or connexions previously made tend of their own motive power, so to speak, to bring back past mental events in some possibly modified

form? This is the conventional doctrine of association, or, in more homely terms, of habit. Or alternatively, is the past experience the inert, but nevertheless organized, material which is utilized in the process of thinking by the dynamic needs, stresses, wishes of the organism? This is the view of Lewin and originally at least of Freud. Or lastly, do the directional factors of thinking embrace a dichotomy, with both habit or bonds, and wishes or task, playing a dynamic part? This intermediate view is that of Ach and Watt, and presumably of Thorndike, though Watt is inconsistent on the point. It is not the purpose of this paper to make an exhaustive analysis of this problem, and in any case such analysis would be premature, for it is experiment alone that can decide the issue. It is, however, in place to point out that, rejecting pure associationism, each of the remaining alternatives has its own peculiar difficulties. If we adopt the Würzburg view that the mechanism of thinking has a double drive, that inherent in habit and that inherent in motive, then intricate problems arise as to the relation between the two. To elaborate an example already given: I wish to find the number of postage stamps in a sheet 7 stamps wide and 9 long. I say 7 times 9 are 63. Now in Thorndike's terms, there are many bonds between 7 and other numbers and words. There is 7 and 1 are 8, 7 and 2 are 9 and so on; also 7 days in the week, 7 ages of man, 7 pillars of wisdom, as well as 7 times 4 are 28, 7 times 5 are 35. Under any theory involving an associational background all these associations must be somehow involved when one says $7 \times 9 = 63$. We have seen that Ach's hypothesis of the determining tendency maintains that they are all inhibited, while the correct one is reinforced by the determining tendency. This may be so; but it does seem to be an extraordinarily wasteful method. It means that the major work involved even in such a routine process as the calculation of the number of postage stamps is spent not on the making of the one correct answer but on the inhibiting of the multitudinous incorrect ones. The emphasis seems to be in the wrong place. Somehow, it reminds one of the Irishman's definition of the way to make a cannon, by taking a hole and pouring some lead around it. The Würzburg doctrine has already been criticized because it depends on the external doctrine of reproduction. The present difficulty constitutes a further obstacle which the Würzburg doctrine must meet even though it can be so modified as to avoid the implication of reproduction.

On the other hand, to deny all dynamic force to habit as Lewin does seems equally difficult. Both experiment and common sense seem to point unequivocally to cases where sheer unmotivated habit persists simply because it is habit. One thinks for instance of the stereotypes,

useless movements, in the animal mazes and of habitual mistakes in typing and speaking. All of the latter are surely not to be explained along early Freudian lines. That every thought and action is motivated by the immediate urges of the mind is indeed a proposition of Freud's which has aroused considerable opposition. Freud himself has later postulated a tendency towards reinstatement. It is conceivable that experiment may reconcile the paradox of the two opposing difficulties along Lewin's lines by establishing the proposition that the difference between motivated and unmotivated processes is one of degree only, motivation being a convenient term for a more inclusive integration. Even then, we should still have the problem of the relation of integration to that which is integrated.

It is worth noting with reference to the contribution of Ach and Watt that objective psychology has here again found itself travelling much the same path as subjective psychology a generation ago. Leeper's experiments on animals have caused him to distinguish between habit and the utilization of habit. As the result of training, his animals turned to the right or left at a T according as they were hungry or thirsty. "Motivation", he says, "can exercise a directive function in relation to habit utilization" ((12), p. 38). With appropriate changes of terminology, this sentence might have been written by Watt or Ach. Leeper is apparently unconscious of the relation between his experiments and the Würzburg work for he does not mention the latter. It is perhaps further worth noting that many maze workers have noted the fact of variability in performance from day to day. A day's run of the maze is very obviously not an exact reproduction of yesterday's. Certain investigators have indeed gone so far as to postulate a principle of spontaneous variation. This variation was easier to observe in the maze than in the conditioned reflex experiment, which is a limiting case, chosen to support the doctrine of behavioural reproduction; just as the corresponding limiting case, the nonsense syllable experiment, was used in support of the theory of the reproduction of experience.

The question of the direction of thinking may now be specified a little more closely. It becomes clear that we have in reality to do with two distinguishable groups of problems, or possibly with two aspects of the same problem. There is first a group of questions concerning what Lewin has called the motor of thought. What is the energizing principle by which successive thoughts or stages of thought arise? What causes the train of thought to move? There is secondly, a group of questions concerning the relevance of the processes when they do appear. What keeps mental processes relevant? Conceivably the energizing principle and the

directive principle may be identical, but we are not entitled to assume this. For there might possibly be called into being a train of mental events which were irrelevant both as regards any external situation and as regards each other, that is, a train which was energized but not relevant. Though the results of experiment, rather than of common sense experience show that this is unlikely or at least very rare.

It may be seen that the association psychology derived both energy and relevance from the single principle of association or, in the later terminology, from the reproductive tendencies. Watt and Ach derived the energy mainly at least from reproductive tendencies, and explained their relevance by the *Aufgabe* and determining tendencies, though Watt is not entirely clear-cut on the point. Once again, the problem of mechanism encroaches on that of material; for while the question of energy is fairly clearly a question of mechanics, so to speak, the problem of relevance, as involving the nature of the processes that are brought into being, involves the question of material.

(2) *The theory of Selz*

The historical successor of the Würzburg school was Selz(13), who indeed numbered Külpe and Bühler among the subjects of his experiments. He was the first experimentalist to reject the conventional association explanation of thinking altogether. It is well known that for Selz thinking consists in the determined completion of a unitary *Wissen*, or knowing, or in the case of productive thought in the determined initiation of certain mental operations which he claims to be of a reflexoidal kind. The nature of what he calls the determination is obscure in Selz's account. What he does is to say in effect that the energy comes from the determination. But of the nature of such determination we seem to be told nothing, nor whence it comes, except that it is a factor in a unitary task, which is more inclusive than Watt's task. In another passage he identifies it with a modification of the will. In fact, Selz's solution of the problem why thoughts arise is the purely verbal one of saying that the task contains a determination. The non-explanatory nature of the statement is in large part concealed by the fact that he has borrowed the term determination from his predecessors, who had used it in an associational context. If the determination does not function by reinforcement and inhibition of reproductive tendencies, one is, however, justified in asking how does it function. It is really to the problem of the relevance of the processes that Selz has devoted his great acumen and industry, leaving that of the cause of their appearance unsolved and really unadmitted. Concerning his

solution of the problem of relevance no statement will here be made except to say that there are grave difficulties in a proposed mechanism for thinking which uses the reflex of Sherrington for an analogy and gives the pupillary reflex and decapitated frog as illustrations.

(3) *The Gestalt theory*

The other well-known non-associative scheme is, of course, that of the Gestalt school. For the members of this school a problem situation is one the perception of which sets up certain psycho-physical stresses in the organism. Very roughly speaking, thinking is the process by which these stresses are resolved. This process is, however, not one involving the present and its immediately engendered tensions alone; it works in conjunction with and through the trace-column, the masses of traces, ultimately of a chemical nature, left by past experience ((14), p. 207). During the process of release reorganizations of the total field occur which correspond in experience to what is known as insight. "Such movements as would increase the total tension in the field are excluded, and the dynamic situation demands such movements as will decrease the tension" ((14), p. 626). The stages by which thought proceeds are relevant because, the perceived problem being equivalent to a system under stress, such transformations occur as are specific to the resolution of the particular stress in question. The whole series converges towards the specific equilibrium demanded by this particular problem. The necessary energy is provided by the tendency of the system under stress to achieve a state of lower tension, like the mainspring of a watch. So that hypotheses are offered to account for both the relevance and the energizing force of the thinking.

It must be said that after reading the accounts given by earlier workers the Gestalt hypothesis is highly attractive. It has an organic quality not possessed by any of its predecessors, and one which seems to be more in keeping with one's actual experience and with modern ways of regarding the organism at work. But when it is examined, very grave difficulties appear. Of these I shall mention two only.

The hypothesis assumes that a certain quantum of energy resides in the original system under stress. When the stress is resolved, a lesser quantum is present, the difference being due to the energy consumed by work done. The argument thus implies a closed system, where the energy used in doing work plus the energy present after the work is done together equal that originally present. The physical postulate of the conservation of energy of course assumes a closed system. In fact, Lewin himself

postulates "a greater or less degree of abscission" for his tensional systems. But clearly these systems-under-stress occur in the organism and are in fact partial systems of the organism. So that we have the notion of a system which is to be considered as a closed system, and which at the same time is a partial system of the organism from which it momentarily derives its energy. It is not impossible that the logic of such a state of affairs may ultimately be worked out. The body, in its wisdom, as Cannon would say, does preserve many constants in what may be regarded as relatively closed partial systems. Such, for example, are the constants involved in the composition of the blood. But we are far in psychology from the exact quantitative data and methods of the haematologists. It will be noticed that the psychological hypothesis under discussion is ultimately of a quantitative nature, in spite of Lewin's general disclaimer.

That the above is not merely an academic difficulty may be shown by the single illustration of errors. To make a mistake must use energy, just as much as to take a correct step. It should then be possible to discharge the system by making enough mistakes. Perhaps it is. But every mistake should diminish the original tension. This is contrary to experience; a mistake often seems to increase the tension. Koffka would say perhaps that there is here reinforcement from the ego system; but this would mean that the tensional system was not closed, as assumed. Once again, it is not suggested that these and other similar difficulties are insurmountable, but only that they have not yet been surmounted.

In view of what has already been said concerning the relation between the psychology of thinking and objective psychology, it may here be mentioned that both Selz and the Gestalt psychologists have used objective material in support of their hypotheses.

IV. Conclusion

This highly condensed survey of the explanations which have been given of the determination of thinking may seem to be very inconclusive. Pure associationism has been shown to have its difficulties; the Würzburg modification has others; the non-associational schemes of Selz and of the Gestalt school have equally been criticized. No satisfactory alternative has been suggested. So that the problems have, in a sense, been left hanging in the air. In extenuation it may be said that the history of the subject in the last fifty years has largely consisted in a rebellion against the associational theory, but a rebellion which has never quite succeeded.

And the account of an unsuccessful rebellion against an hypothesis felt to be unsound must necessarily have a certain inconclusive ring about it.

Nevertheless, it would not be true to say that nothing positive has been achieved. For the whole field has been progressively clarified and its details sharpened. When, for instance, we see the objectivists finding themselves involved in much the same difficulties and offering much the same solutions, in much the same sequence, as their predecessors a generation ago, we may rest assured that these problems and difficulties belong to psychology itself, not to the use of a faulty method. Further, at least two fundamental questions have been thrown into relief as a direct result of the work described. The one concerns the relation of past experience to the direction of thinking. The other is very closely related; it concerns the relation of motive to the processes energized by motives. Minor problems have also been clarified, hidden assumptions have been brought into the open. If the work of fifty years has not solved the major problem, it has at least defined it and shown where the difficulties lie. And that is perhaps more than half the battle. One who believes in our science may perhaps be pardoned for expressing the conviction that, although it is as yet unsolved, the riddle of the determination of thinking will ultimately yield to experimental attack; and that the successful attack will be made not by behaviourists nor yet by introspectionists but by—Psychologists.

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SOCIAL PSYCHOLOGY AND SOME PROBLEMS OF ARTISTIC CULTURE

By R. W. PICKFORD

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I. Psychological tendencies expressed in painting

It is particularly in relation to the problems of artistic, religious, scientific and the more highly developed forms of ceremonial culture that the inadequacy of many biological and psychological theories is felt most keenly. If we turn to painting as one aspect of artistic culture we find that painters often express aggressive,[1] destructive,[2] and rebellious[3] tendencies both in the subject-matter of their paintings and in their brushwork[4] and technique.[5] Many other tendencies are often expressed, but these are mentioned because they are sometimes overlooked. The work of art usually mirrors aspects of the artist's mind,[6] conscious and unconscious, as well as aspects of his cultural environment.[7] Where rebellious and destructive tendencies are not clearly apparent in a work of art, we often find their compensations—excessive perfection and integration.[8]

[1] E.g. Toulouse-Lautrec, *The Ringmaster*. Angelico, *Martyrdom of St Côme and St Damien*.

[2] E.g. Millet, *The Wood Sawyers*. Géricault, *The Raft of the 'Medusa'*.

[3] E.g. P. Breughel the Elder, *The Unfaithful Shepherd*. Delacroix, *The Barricade*.

[4] E.g. van Gogh, *Flowering Chestnut-trees*.

[5] E.g. Paul Nash, *Sunrise: Inverness Copse*.

[6] E.g. R. Mednikoff, *Head Waiter* (Read, H., *Surrealism*, Pl. 52). Paul Klee, *Mask of Fear* (Read, H., *Surrealism*, Pl. 43).

[7] E.g. Master of the Half Lengths, *Three Musical Ladies*. A. Rubliev, *Three Angels*.

[8] E.g. M. Laurencin, *Girl with Rose*. P. de Chavannes, *The Fisher Family*.

Even what is marked in a work of art by its absence may be of outstanding psychological[1] or aesthetic importance.[2] The balance of tendencies in the artist's mind is expressed in the balance and design of the painting,[3] and the person who appreciates the painting usually likes or dislikes it according to the degree to which it is appropriate to his own peculiar mental organization. The artist's conscious and unconscious mental outlook is based on his own motives and tendencies and reflects his social environment which depends on the biological tendencies of its group life. It is clear, therefore, that many psychological and biological ideas about social groups are closely related to artistic culture, in spite of their apparent inadequacy.

II. Fundamental motivation tendencies

Fundamental human motivations seem to be grouped round four chief modes of biological relationship between the organism and its social and physical environments: (1) food seeking and alimentary; (2) defensive and aggressive; (3) sexual and reproductive; (4) comradely and social in so far as not implied by the other three. The exact number of particular tendencies may be difficult or impossible to determine. These modes cannot be treated as stereotyped reaction patterns or as mechanically separate classes of response. They seem even to defy logical analysis, because the relationships between them, at least in man, must be worked out more in terms of emotional meanings than of objective reaction patterns. The meaning of food seeking, for instance, may from the very start be bound up with sexual and comradely meanings, and with the desire for protection, and may easily come to involve defence or aggression. In the same way, the fundamental social tendencies of assertion, comradeship and submission are often linked up in an intimate way with sexual, defensive, aggressive or feeding tendencies. At the same time, it does not seem to me possible to say that human motivation is all attributable to an originally undifferentiated *libido*. We have to consider what modes of relationship with the social and physical environments are chiefly represented in a given human response or interest, and how that particular expression of them has arisen in the life of the individual concerned. These problems can, if we insist, be treated in terms of the

[1] As pointed out recently by Dr K. M. Abenheimer. Cf. the anonymous painting 'Abstraction' (16).

[2] Cf. Schubert's Unfinished Symphony.

[3] E.g. Matisse, *Woman before an Aquarium* (showing conflict). Velasquez, *Venus and Cupid* (showing harmony).

individual alone, but in fact they always imply social psychology, because the cultural and social patterns must be taken into account as conditions of response. Human behaviour and motivation are tremendously influenced by the cultural pattern and social environment, and, although the fundamental motives may be instinctive, actual behaviour can never be explained solely by reference to specific instincts.

It is necessary to point out that these ideas must be treated as dissecting instruments for the investigation of behaviour and experience, and must not be allowed to become a scheme of faculties which are supposed to explain the living organism. It is always the group or the organism which 'tends', just as it is the organism which perceives, feels, remembers, or imagines, and it always does so in relation to particular conditions or circumstances. The objections which are now frequently raised against the instinct theory are sound because that theory has been misused. They are properly directed against the misuses and not against the theory, for psychologists have been inclined to forget the organism in talking about the instincts.

It is interesting that the psychological principles underlying aspects of advanced cultures, such as painting, are not widely different from those underlying primitive beliefs and practices. This, although many people will say it was obvious from the start, is important because it is by no means generally accepted. Many people, after admitting the excellent qualities of primitive art, habitually assume that their own great artists are in possession of realities altogether inaccessible to primitive man. Indeed, it is well known that logic is able to prove whatever the logician believes to be true. It would not be surprising to find that other branches of material culture and learning developed and expanded by psychological steps not remote from those of art.

III. Tendencies expressed in painting and their relationship to particular material

There are two very interesting questions: How far is painting the expression of unique and how far of general tendencies; and how far do those tendencies express themselves by using material peculiar to them? A moment's reflexion will show that the number of tendencies concerned in painting, and their variety, are very great. Many of them, such as vanity, may be expressed directly. Others, such as the desire to make money, are involved indirectly. There is, however, no simple fixed division between the tendencies which are directly and indirectly expressed, and in practice the complications are very great indeed. If, however, we whittle

away one by one the tendencies which, though often involved indirectly, are not of direct importance either to the attitude of the artist when painting or of the person who enjoys pictures, it may be possible to reach a minimum which is absolutely essential. Such a minimum would be the indispensable nucleus of artistic expression and valuation, and the problems of its existence and how it may be defined, are very difficult. Such problems cannot be dealt with here, but it would appear that the absolute essentials in artistic expression and appreciation are better described in terms of the harmonious working together of other tendencies than as unique aesthetic tendencies themselves (cf. (1)).

So far as the evidence can indicate, a vast number of aesthetically unessential tendencies are normally involved in painting, but they are psychologically essential to the particular form which painting takes in given circumstances. The Russian icon may have taken its particular form because of the special combination of tendencies in its religious setting, but these religious tendencies may be aesthetically unimportant. In this article, which is not a study of aesthetic values, no evidence has been collected about the essentials of aesthetic attitudes. Painting appears to be an everyday interest, and those who think art does not contribute to our everyday well-being are probably in the wrong. It does contribute to ordinary life, and is normally present in every culture which is not, from some cause, specially narrow.

The question of the special material, if any, dealt with in painting, requires a similar treatment. If an essential 'material' can be found, it will probably be best described in terms of harmony of visual rhythm and design. Visual art, however, may express its designs in subject-matter related to any tendencies normal and abnormal, and therefore may be relevant to any aspects of ordinary life.

The problem of the relationship between the tendencies expressed in painting and the material they deal with is of great interest, but again, exceedingly difficult. The full complexity of this problem is brought out when we consider, for example, the Russian icon (2), Rajput painting (3) and Bushman art (4). All of these, without doubt, are largely of religious import. None of them is intelligible to us in its original meaning any more than West Christian iconography can be to a normal Chinaman. It is only by a laborious intellectual reconstruction that the relationships between Bushman drawings and the tendencies which were expressed in their making can be seen even dimly. Indeed, for Bushman and Rajput painting and the Russian icon, we are hardly in possession of the data necessary to make this reconstruction at all. Even where such material

is more available, as in the pre-Raphaelite movement, much difficulty is found, and attempts to take, in imagination, the place of the artists are exceedingly precarious.

In spite of this, however, the appreciative observer is able to see the genius in all these forms of art, and Bushman paintings are comparable in fascination with Russian icons. It is tempting, therefore, to conclude that the most central core of tendencies in painting does have a close relationship to the material—harmonious and rhythmical designs—in which it is expressed, but that the less relevant tendencies, connected with religion, warfare and everyday life, are related to their subject-matter mainly by the accidents of circumstance. It is accidental that the Russian icon deals with Saints and the Virgin Mary, and that Bushman painting deals with rainmaking and the springbok. These aesthetic accidents, however, are circumstances of the greatest interest for the history of culture. Further discussion of the existence of essentially aesthetic tendencies or material, and of the relation between them, will unfortunately have to be avoided.

IV. Influence of the super-ego

In psycho-analysis the 'super-ego' is conceived as a complex of tendencies largely unconscious, arising from the individual's social contact with his parents and other adults, since infancy, and expresses, in the main, his capacity to control himself in such a way as to fulfil what he feels to be their demands. Often there is much conflict between the demands of the super-ego system and the demands of 'reality' and those of the persons's own instinctual life; or the relationship may be ambivalent —partly made up of conflicts and partly of acceptances. Any persistent tradition handed on from master to pupil or followers might be explained by saying that the master had entered into the super-egos of his followers and thus determined the direction of their interests. Any obstinate rejection of cultural influences or traditions which have become specially identified with the names of great masters, conversely, may be explained as due to reaction against the control of the hated super-ego. It might be said, that in Cézanne's(5) artistic development these two influences are present. Sharp antagonism to his father and to domestic control and discipline of any kind was transferred to social restraint and to cultural conventions: he became a violent artistic rebel. On the other hand, the relationship with parental control was not without its positive aspect, and, where not presented in direct form, cultural tradition had a strong

attraction for Cézanne. He copied paintings in the Louvre and chose his own masters—Daumier, Chardin, Poussin and others. Thus the rebel fell into what was actually a very strong tradition, and became its greatest living exponent, though long unrecognized.

It is necessary only to mention Rossetti's enthusiam for Dante and early Italian literature and painting, combined with his rejection of his father's direct influence; Corot's objection to becoming a draper like his father, together with his dependence on the very tradition that later influenced Cézanne; the extremely close connexion between Russian icon painting and the social and cultural authority of the Eastern Church; and the collapse of Mogul miniature (6) painting when the personal influence of the Indo-Persian emperors was withdrawn, to remind the reader that there are innumerable events in the history of painting which might be explained by the super-ego theory.

It may be said that Mogul painting was due to a combination of Indian and Persian traditions under leaders who entered into the super-egos of their followers, and that the Persian tradition itself was carried forward to that point by a series of regal ancestors of these leaders, from the time when the Mongols conquered China in the thirteenth century. These ancestors were all patriarchs of nomad hordes, who might awaken admiration, envy and resistance in some of their followers, in accordance with the super-ego theory. Feeling inferior and barbaric in China, these ancestors had borrowed her culture for prestige, and carried it to other parts of Asia. China, however, is the great land of ancestor worship, and it would be easy to argue that the very essence of Confucianism is the combination of fear and adoration of the dead, and that every dead man worshipped is a symbolic super-ego, who watches closely over the living to see that everything is done in exact accordance with the conventions of many hundred years standing. The Mongols might be said to have borrowed unconsciously the super-egos of the Chinese and to have carried them to India. This theory may account for persistence of tradition, but a weakness is clear when the discovery is made that the outstanding period of artistic and other constructiveness in China was part of the T'ang Dynasty (A.D. 618–905) when religious freedom existed. Waley[1] points out that the three centuries of this dynasty were characterized in turn by Buddhist speculation, by lyric poetry, and by the Confucian revival. In the eighth century A.D. Manichaean Christianity existed side by side with Buddhism and Zoroastrianism, and this part of the T'ang Dynasty was China's greatest period of outside contacts and expansion. In A.D. 845

[1] Cf. (7), Chaps. VIII–XII; also Giles (8).

the conservatives triumphed, and Christians, Zoroastrians and Buddhists were suppressed, with consequent destruction of paintings. The growing interest in romantic landscape painting after that time may be traceable to desire to escape the rigours of Confucianism. Only after the Confucian revival set in did the nation become isolated and gripped by convention, and ever since cultural construction has been steadily on the decline. The super-ego theory has some importance, especially in connexion with conservation and cultural decline, but, clearly, other principles will have to be used to account for constructiveness.

V. Repression and sublimation

A complex system of processes, conveniently called sublimation, seems to be very important in painting. It is widely recognized in psychology that tendencies, usually in the form of wishes or desires, which conflict with other tendencies in the same person, or with social influences expressed in general social pressure or in the will of other persons, may be subject to unwitting repression. Such repression is one way in which the organism naturally protects itself against overt conflicts, sometimes with disastrous results. Repressed tendencies usually persist unconsciously and may express themselves in disguised form as dreams and symptoms of various kinds. Feelings and emotions may pass into the unconscious in the same way, and reappear in disguised form also. The degree of repression and the nature and extent of the disguises may be very varied indeed.

Much of the content of pictures is disguised material which has been subject to some degree of repression, and many of the special interests of artists in painting may be due to repressed tendencies. If this process of indirect and sometimes refined expression of crude but disguised tendencies and material is called sublimation, then sublimation is important in the psychology of painting. It is interesting to observe that the ultra-modern art of the Surrealist group, judging from the Surrealist Exhibition in London, 1936, differs from normal art rather in degree of disguise than in the actual content of sublimations. In a conventional nineteenth-century painting a bull, a cow and calves may be shown, standing near a pond under some trees. The animals have a distinct hint of the human about their faces, and the ensemble is carefully arranged so that the sexual organs of the bull are not to be seen. A Surrealist painting might show a life-size human arm with fist clenched, not draped but thrust up to the elbow through a wooden quoit ring painted scarlet. Whatever philo-

sophical or aesthetic meaning the painter may find in his work, from the point of view of the sublimation theory the essential difference between these two pictures is that the symbolic transformations of the sexual *motif* in the bull and cow painting are far more complete than in the other, which is all but a plain rendering of the penis inserted into the vulva. Each subject, however, gives the painter great opportunities for fascinating use of design, rhythm and colour, and that may be its true, artistic meaning.[1] As already pointed out, it would be rash to assume that the sublimations and symbolisms in particular paintings could be readily and fully determined. It is clear to us that Rossetti's interest in painting his sister, his wife and Mrs Morris may have been relatively simple sublimations of sexual desires directed towards them. Rossetti may or may not have been aware of this. Most painters, however, who are not altogether stupid, must realize that every drawing of the nude is a sublimation. On the other hand, the full nature of the sublimations in Daubigny's paintings of the Oise, to the scenery of which residence with his nurse at Valmondois had introduced him, will probably remain as big a mystery to us as it would have been to him had he been asked to explain it.

Clearly, sublimation itself is the indirect expression of social influences, or of the conflicts they engender in the individual, and to explain the part it plays in artistic constructiveness requires also the explanation of those influences. Though every painting of the nude may be a sexual sublimation, the human figure has a value because it gives unique opportunity for the construction of rhythmic and harmonious designs. The important points are that sublimation itself does not explain cultural constructiveness, and that to discover a painting to be a sublimation of certain tendencies does not fully explain its artistic interest.

In this study it is tempting to cite instances, such as that of Goya, who is said to have had twenty children in wedlock, which seem to disturb Unwin's (11) theory that cultural constructiveness is directly the product of sexual restriction. While little support may be found for this theory, however, it would be unwise to lay very much stress on such instances. Goya's reproductive and his pictorial fecundity may have been a dual

[1] This limitation would be denied by the surrealists themselves. "Completely devoted to the imagination and its charms, surrealist poetry aims at exteriorising all man's desires, all his obsessions and his despairs: it gives him the means to free himself, to venture forth." "But I have only shown one of the aspects of Surrealism. There is, in addition to its poesy, its social function, its political function, its complete adherence to the principles of dialectical materialism, its revolutionary position, and its struggle against patriotism and the bourgeoisie." See Read, H. (9) pp. 218 and 246; also (10) Ch. v, where there is a discussion of the Freudian theory of the artist.

expression of the same repressed desires seeking their outlet. Great sexual energy is often an expression of passionate struggle against repressions rather than the product of straightforward desire. Unwin's theory must be investigated by a detailed study of special case histories.

VI. The Gestalt theory

It is not necessary that the Gestalt (12) theory should be discussed at length, because most readers will be quite familiar with the general meaning of this theory. In social psychology it depends chiefly on stressing the configurational aspects and the patterning of social groups. In the history of art, for instance, the origin and development of either the Barbizon or the pre-Raphaelite groups might be expressed in Gestalt terms. Obviously, however, the question of absolute origins must be put on one side, as leading back to infinity in social history, and every individual must be treated as approaching the particular social developments being studied, with his own frame of ideas and his own tendencies already socially determined. In either the pre-Raphaelite or Barbizon groups certain individuals, with such predetermined attitudes, ideas and tendencies, are found to meet through circumstances more or less unconnected with their particular artistic aims, and very quickly make personal contacts, as a result of which more permanent attachments, and then a special group, are formed. The development of these attachments can be well described in terms of Koffka's principles of the similarities of moods and emotions among persons who may or will form a group, and of the 'incompleteness' of the isolated individual. The main characteristics of the groups formed can also be expressed in his terms. Each group rapidly becomes a unified system, segregated from its social background, stable and articulated within itself. The degrees of unification, stability, segregation and internal articulation differ in any two groups, but these characteristics are always present in some degree. Koffka's treatment of the products of social activity, the mass of material and ceremonial culture together with traditions and social habits, is also easily applied to such groups as the Barbizon and pre-Raphaelite painters. Each developed in a special social setting, selected and rejected aspects of that setting, and constructed a new phase of culture upon the framework of what was selected.

In all these ways, the Gestalt theory is not remote from the theory of individual and social tendencies. Apart from purely verbal differences, the two views are closely comparable. There remains, however, the diffi-

culty of Koffka's claim that psychology must seek the explanations of its data outside its own realm; in short, that it must find those explanations in physics. This is an *a priori* claim, which remains to be tested like any working hypothesis, and is opposed to the commonly held view that, in the study of behaviour and experience, mental explanations are required. It is not the aim of this paper to weigh the evidence in favour of each view, but the writer thinks that, while the problem is not likely to be settled during the present century at least, the facts of organic life appear to be against Koffka's claim (but cf. Waddington (13)). The constructive nature of many organic and social changes is far from new as a stumbling block for theories of the organism based on physics. Koffka's treatment of the relation between psychology and physics is original, and makes the unification of these sciences appear more likely than it has appeared before. The principle that these sciences ought to be unified, however, is itself a working hypothesis, which has no claim on us except in so far as it may give the most complete and economical explanation.

VII. Biological tendencies

The relations of the organism to its environment are of fundamental importance for biology, and a great number of biological problems arise from their study. The environment, of course, may include other organisms as well as purely geographical conditions. The organism, viewed biologically, is a unit which strives to survive under given conditions, and fails or succeeds to some extent. The peculiar structure and behaviour of any organism have to be understood in terms of this effort to survive and the nature of the environment. This proposition is not fundamentally altered by mechanistic biologists, because they study the same problems, but reject all explanations which are not mechanistic. In the study of groups we are concerned with biological questions. The social group, again, is a unit which maintains itself with more or less success, under given conditions which include other groups as well as geographical circumstances. As MacCurdy (14) points out, the group itself 'strives' towards survival, in a certain sense; and what happens to a given group is to be understood in terms of its striving, its structure and the external conditions for and against it. A group usually has a certain period of activity and then fails to maintain itself. This is also what happens to all collections of organisms which are sufficiently integrated to have one or more characteristic social tendencies, even if not integrated enough to appear as sharply marked groups.

Since it is concerned with the effort of the social group after survival and with the conditions under which this effort occurs, the biology of groups is difficult in a particular way to apply to the history of painting. Mankind would appear to be partly controlled by tendencies other than those immediately related to survival value, else activities so remote as painting from self-protection, reproduction and feeding would not be included in the everyday life of human groups.

It is worth considering just how deep is the separation between painting and these biological tendencies. Many painters have successfully earned their livings by their art; there is no simple connexion between ability to paint and sexual impotence; and most painters are quite as capable of defending themselves as are other ordinary members of society. There is no evidence that the best painters are less able to earn money, less sexually active or less able to look after themselves than bad painters. Indeed, though it is often called a 'fine' art, painting is comparable with any good trade. There is a less deep gulf between it and food seeking, reproduction or self-protection than many people think, for it is able to combine effectively with any of them. To this warning for people who might be inclined to overstress the idea of pure art, however, it must be added that among the interests of mankind there do seem to be some which are not strictly biological, and the interest in art is among them. It is clear that biology cannot fully explain human life, just as physics cannot fully explain biology. Nevertheless, the biological aspects of human life are of great interest.

VIII. The individual and the group

If we turn to the position of the individual, it is futile to regard social life as created acccidentally by the coming together of individuals. Social life is not even something simply thrust upon the individual by circumstances unrelated to his active tendencies. On the contrary, he is incapable of being happy without it, and normally adopts a constructive attitude towards it. The relationship of the individual and the social group is always biologically complementary in essence, and only when failure and infantile and other conflicts have set up acute discords, is the attitude of the one to the other destructive. In fact, biologically, social life never has a beginning for the individual at all. It arises together with the bodily relationships and organic continuity of parents and offspring. Solitary young do occur in the animal kingdom, and young which are normally neglected by the parents but which form groups on their own

are frequently found (15), but neither condition occurs in mankind or any of man's near relatives.

The development of social life, considered from the point of view of the individual, is a process of differentiation, in which the simple possibilities of infancy become elaborate and complex. In this elaboration the attitudes of the individual become more specific, and he selects parts and rejects other parts of the customs and cultural material of his own and other social groups. He is then the focus of constructive changes going on in his group. During this development he may meet with frustrations so great that he loses touch with surrounding changes, but since recluses like Cézanne may be highly constructive, there is no simple relationship between conviviality or obvious public helpfulness and social construction. From the point of view of society the individual is a special focus of activity and does not require to be driven into a useful path, but, if forced to do what will normally be his own desire, he may become destructive and rebellious. What is true, in this way, of the gifted genius, is true in due measure of the ordinary person.

Obviously, a very important biological function of small groups, which, like the pre-Raphaelites and the Barbizon painters, exist within a wide social setting, is that they support the individual during the period of his most difficult struggles. This kind of group is a stepping stone to wider social activities. While the members need the support of immediate friendly relations, they stay together, but when this need has been fulfilled and is past, or when their interests have developed and changed, they find some occasion, probably trivial and such as might have been passed unnoticed before, and quarrel.

Groups, however, must not be viewed only from the individual's point of view. They are also organic units which pursue a certain course of social constructiveness and rebellion, and are formed at times of particular coincidences only one set of which are the desires and needs of individual members. There is much to support the view that, if those particular persons had been absent or different, a group might have been formed on broadly similar lines with another personnel. This would be possible only in the presence of able men, of course; it is well known that the loss of a great leader is often fatal to a group movement. However, it is clear that the group itself moves towards social change and construction, and, once formed, carries the members along.

After a period of activity the group or movement may sink before circumstances too adverse to be withstood, or before new tendencies which it cannot assimilate. In this the group fails to secure its own

survival and fails to be a source of inspiration to new members. One adverse circumstance may be that the material dealt with has been at least temporarily exhausted by known methods. Apart from this relatively infrequent possibility, the adverse conditions may be of many kinds. When a group or movement sinks before adversity, it is well known that self-conscious efforts of individual members are not successful in resisting the decline. The individual is powerless where the group fails. Nevertheless, many of the methods, standards and actual products of a movement or group may remain in the hands of successors, and be vital to their progress. From the biological point of view there is no reason why the tendencies of a particular group might not run on to excess in a given direction, and destruction might come through a sheer overloading of influences and material originally helpful and adaptive. The only essential condition of this is that there should be no destructive adversity. A vast number of tendencies are preserved in our own society merely because there is nothing strong enough to destroy them. In all these and many other ways the painting group is based on a biological footing.

IX. Conclusion

In conclusion, it seems that the problems of artistic culture must be considered in the light of psychological knowledge of the individual and the social group. The individual's chief tendencies in relation to his social and physical environments are food seeking; sexual and reproductive; defensive and aggressive; and comradely. These are not mechanically separate 'drives', but are tendencies emotionally interrelated in complex ways peculiar to each individual himself. Painting expresses many and varied interests, but there seems to be no specific artistic tendency by itself. If there is a specific aesthetic material dealt with in art, it seems to be the rhythm of pattern, design and colouring, but even this is always intimately expressive of the artist's personal life in symbolic ways (16). Strictly speaking, 'abstract' art is non-existent, and all art is really full of meanings related to everyday life, expressed with varied degrees of clarity.

The Freudian process of sublimation is important for the psychology of paintings, for every picture is a sublimation, usually of many tendencies organized together. The super-ego is also important, and much of the influence of persistent traditions may be explained by it, though social constructiveness seems to require other explanations. The social group of artists, which is often an important aspect of artistic culture, is like a

biological system which takes part in the struggle for existence, and which supports its members through periods of social and personal difficulty. In turn it expresses the main tendencies of the wider cultural environment of its time.

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FATIGUE IN TERMS OF THE ELECTRODERMAL RESPONSE

By CHRISTIAN A. RUCKMICK

I. *Fatigue as a topic for experiment* (pp. 211–213).
II. *Early experiments* (pp. 213–215).
III. *A revision of method* (pp. 215–218).
IV. *A suggested hypothesis* (p. 218).
References (p. 218).

I. Fatigue as a topic for experiment

From the study of symptoms of mental fatigue made by the psychiatrist Sikorski (1), in 1879, to the carefully controlled experiments of Lapicque, is a matter of only half a century. During this period hundreds of books and articles have appeared considering the subject from many angles, and the list of contributors would be equivalent to a roll-call of the most prominent psychologists and physiologists of the world. As techniques and physiological knowledge have developed, so have the theories, which have been advanced to explain the varied facts, taken on a great many different forms. When in 1891 Mosso picked up quail which in their exhaustion had flown blindly against the tree trunks and the white walls of houses along the Italian beach on their northward flight from Africa, he sought to establish the fact that in "extreme fatigue cerebral anaemia is produced and that this anaemia may diminish the power of vision" ((2), p. 3). Lapicque suggests to-day that fatigue is analogous to curarization. Between these extremes lies a long winding road of progress in technical knowledge based upon the perfection of the scientific tools of research. Already Lapicque's interpretation has been questioned by Lindsley and others, but such appraisals surely spell further improvement in the understanding of the complicated concept of fatigue. How complex in one instance alone the problem of fatigue is has been clearly pointed out by Offner who stated as early as 1910 that "these and similar phenomena are psychic symptoms of physical fatigue, and show us that bodily activity results in mental, as well as in bodily fatigue"((3), pp. 9f.). Later on he reverses the situation by calling attention to the physiological results of

mental activity: "Moreover, the unconscious, or, as they are termed, purely physiological processes, are affected by fatigue through mental work" ((3), pp. 15f.).

Our problem is obviously, then, a psychophysiological one. In that connexion it is therefore not amiss to apply a technique that is rapidly gaining prominence as a form of electrical manifestation of bodily condition and activity, namely the electrodermal response (E.D.R.). Since the sweat glands are tied up chiefly with the widely ramified autonomic nervous system, it should be illuminating to discover how this mechanism reflects mental and physical fatigue.

Further, and more directly in line with other research problems on feeling and emotion in the laboratory, is the need to envisage the manifold factors which are reflected in the electrodermal response. Until we can identify the phenomena which influence the activity of the sweat glands, we cannot say that the responses which we obtain in affective or emotional situations are truly indicative of feeling, or believe that we are holding all other factors constant in an affective experiment. For a series of published researches of mine have shown that we must approach the electrodermal response with due caution. Affective phenomena are frequently indicated but so are many other mental and bodily processes. Movements of certain types, for example, as shown by Schaefer's work (4), are characteristically accompanied by periods of recovery about twice as rapid as those for emotional experiences.

Movements are not the only difficult items with which we have to deal in the affective area. We must study analytically one after another of the contaminating factors in this response. As Davis has noted: "With such wide variability in the reflex the most necessary contribution to knowledge of the reflex seems to be the discovery of more participating factors which can be controlled experimentally. With an increased knowledge of these factors variability would be lessened and further research rendered less laborious" ((5), p. 59). He stated that most attention had been given to "the discovery of bodily processes concurrent with the reaction" and almost no attention had been directed to "research on the mechanisms of conditioning, adaptation, facilitation, fatigue, etc., by use of the galvanic reaction" ((5), p. 8). He therefore proceeded to study several of the conditions accounting for individual variability, especially that of facilitation. For some time we have been impressed in our work with the effect of such general mental and bodily states in our observers as euphoria, relaxation, attention, tension, and fatigue on the recorded deflexions. Bagchi & Greenwald (unpublished work) have shown that

electrodermal changes which are usually associated with involuntary activity can be definitely modified by different types of attention, or mental set, and that these types of attention or mental set can be roughly identified by the varying degrees of conductivity resulting from them. Strained attention is accompanied by a greater degree of variability in dermal conductance than quiet attention.

II. EARLY EXPERIMENTS

Our first direct, although preliminary, attack on one of the persistent problems involved in many, if not most, investigations in the affective area, namely, that of mental and bodily fatigue, was published seven years ago (6). At that time we did not fully realize that the Hathaway apparatus would not give us dependable quantifiable results. The galvanometric readings, relatively crude as they were in the earlier days, did indicate, however, that there was a general tendency toward a consistent decrease of bodily resistance during the state of fatigue (Tables I, II). In many of the simultaneous readings taken with the Hathaway apparatus

Table I. *Average galvanometer readings (Wheatstone bridge settings)*

Observer	Normal	Fatigued	Type of activity
P	38000	43000	Mental and physical work
P	33500	11690	Mental and physical work
P	8800	8700	Mental and physical work
Jk	6290	30390	Long walk
Jk	32790	88990	Mental and physical work
Th	4500	15090	Long walk
Wb	88000	64000	Mental and physical work

Table II. *Fatigue series*

	Galvanometer readings			Systolic blood pressure		
Observer	Normal	Fatigued	Increase or decrease	Normal	Fatigued	Increase or decrease
R	102	180	+	118	112	−
O	60	55	−	106	116	+
R	91	54	−	114	120	+
R	9	6	−	112	118	+
R	142	161	+	112	122	+
O	46	43	−	120	112	−
R	84	46	−	118	120	+
R	75	68	−	116	104	−
O	55	65	+	106	102	−
O	95	77	−	106	103	−
W	68	74	+	106	110	+
W	73	60	−	85	105	+
W	80	60	−	106	94	−
R	32	24	−	112	126	+
R	112	122	+	120	120	
			5 + 10 −			8 + 6 −

we obtained contradictory results, i.e. results which showed an increase in bodily resistance and were therefore opposite in sign to those obtained with the D'Arsonval galvanometer (Table III).

Table III. *Normal* v. *fatigue conditions*

	Galvanometer				Hathaway			
Observer	Normal	Fatigued	Difference	Increase or decrease	Normal	Fatigued	Difference	Increase or decrease
A	3500	2600	900	–	37	50	13	+
A	2600	900	1700	–	60	37	23	–
A	1600	900	700	–	32	39	7	+
B	4500	2600	1900	–	39	12·5	26·5	–
C	3300	2500	800	–	30	40	10	+
C	2500	1300	1200	–	43	38	5	–
D	2500	1500	1000	–	51	42	9	–
D	4000	3300	700	–	30	33	3	+
D	2200	1300	900	–	35	44	9	+
D	4100	2900	1200	–	40	47	7	+
D	1420	990	430	–				
D	1500	1300	200	–				
D	.	.	.	.	88	112	24	+
D	.	.	.	.	94	138	44	+
E	1150	1130	20	–				
F	2949	2620	329	–	143	160	17	+
F	2070	1550	520	–	31·7	36	4·3	+
F	3290	2230	1060	–	30	28	2	–
F	3100	2100	1000	–	41	47	6	+
F	4300	2600	1700	–	40	29·5	10·5	–
F	2510	2300	210	–				
F	9500	8100	1400	–				
F	3050	2610	440	–				
F	.	.	.	.	95	104	9	+
F	.	.	.	.	90	115	25	+
F	.	.	.	.	62·5	40	22·5	–
F	.	.	.	.	46	50	4	+
F	.	.	.	.	55	78	23	+
F	.	.	.	.	30	32	2	+
F	.	.	.	.	39	51	12	+
F	.	.	.	.	24·5	27	2·5	+
G	4200	2700	1500	–	36	37	1	+
G	4600	2400	2200	–	36	34	2	–
G	2800	2000	800	–	35	33	2	–
G	3500	2700	800	–				

Galvanometer readings in ohms of resistance.
Hathaway apparatus readings in potentiometer units.

These experiments were continued in great detail with the Hathaway apparatus and gave us a definitely decreased potential or increased resistance during the state of fatigue in the twenty-nine trials which are described in my recent book (Table IV) ((7), p. 266). Although I questioned the results at the time on the basis of the oft-repeated statements that the Hathaway apparatus could not be depended upon for any quantitative comparisons, it seemed to me that at least the direction of

Table IV. *Mental and physical fatigue*

Trial	Normal	Fatigued	Increase or decrease	Trial	Normal	Fatigued	Increase or decrease
1	62	67	5	16	63	57	−6
2	57	51	−6	17	75	71	−4
3	72	73	1	18	52	53	1
4	55	49	−6	19	51	49	−2
5	59	54	−5	20	58	53	−5
6	48	44	−4	21	63	60	−3
7	78	79	1	22	71	70	−1
8	73	68	−5	23	58	55	−3
9	57	55	−2	24	74	70	−4
10	69	63	−6	25	72	68	−4
11	47	49	2	26	55	51	−4
12	59	53	−6	27	51	49	−2
13	64	60	−4	28	63	60	−3
14	73	69	−4	29	79	73	−6
15	51	49	−2				

All readings are potentiometer readings on the Hathaway apparatus.

change was sufficiently large to justify the statement which I made in my treatise, namely, that fatigue operated in a direction opposite to that of feeling as shown in my Figs. 1 and 2 ((7), p. 367) (see p. 216). In a critical and well-controlled series of trials Forbes showed that the size of proportional neither to actual change in resistance nor to the percentage the deflexion was of change in resistance in the subject's circuit (8). These data, furthermore, corroborated the results reported by Strauss (9), who stated that the rate of recovery indicated the degree of fatigue obtained during the experiment. Seward & Seward (10) showed, however, that a continued negative acceleration in the decline of resistance during an experimental period of 8 min. was symptomatic of the degree of fatigue produced.

III. A REVISION OF METHOD

In order to check my own previous results and the above-cited data I attacked the problem again, using great care to keep as many mental, physiological, and physical factors as possible under control. Analytical introspective records were obtained, the time of day when readings were taken was kept constant, the temperature and humidity were accurately recorded, the shielded sound-proof room was used for all experimental trials, the instructions were carefully followed, and the series of items which were presented to the subject were rigorously carried out. A set of twenty problems in mental multiplication, of the order of 54×17, were assigned to the subject to compute, while painful and unpleasant stimuli were being presented as distractions. These stimuli included penetrating and extremely vile odours like pyridin, carbon disulphide, ammonium

sulphide, asafoetida, and carbon tetrachloride, annoying electric shocks on the tongue, pin pricking, scolding, nagging, and the like, distasteful solutions on the tongue, like acetic acid, and in dilution, citric acid, and muriate of quinine. There were also vigorous exercises, such as lifting the body horizontally from the table on hands and toes up to 20 times, etc. The introspective records gave interesting qualitative pictures of extreme

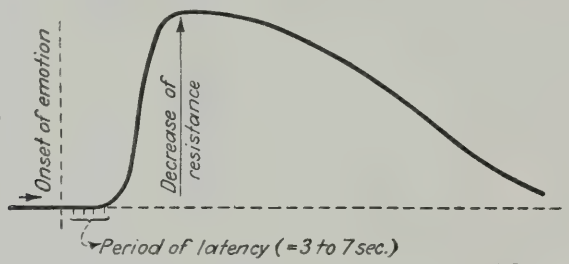

Fig. 1. A typical affective deflection. Reproduced from *The Psychology of Feeling and Emotion,* by Christian A. Ruckmick, through the courtesy of McGraw-Hill Book Company, Inc.

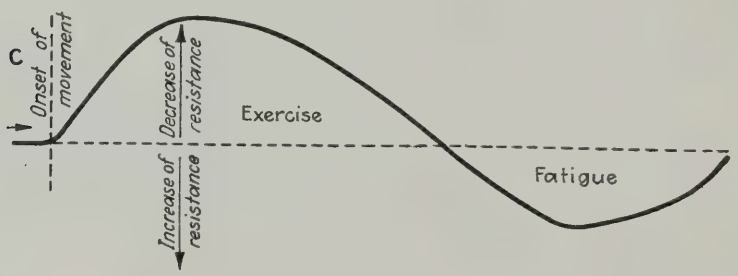

Fig. 2. Curve of fatigue as related to curve of movement. Reproduced from *The Psychology of Feeling and Emotion,* by Christian A. Ruckmick, through the courtesy of McGraw-Hill Book Company, Inc.

bodily fatigue and mental lassitude. Photographs of facial expression further revealed the appearance of a sort of "all-in" attitude. Almost all of the experiments were done on myself as subject because of the somewhat exacting conditions of the experiment. Oddly enough the state of exhaustion which was more complete than that following any mental or physical toil which I have ever "sweated through", did not show any mental effects beyond about an hour after the experimental series.

The results were surprising. The latest improved model of the Greenwald galvanometric apparatus was used[1] and photographic records were

[1] Derived from that described by Greenwald (11); but the present model more closely resembles the circuit described in (12).

made. It will be seen from Table V that almost all the final readings showed decreased resistance as compared with the normal or originally balanced readings. These figures check, therefore, with our earlier galvanometric readings but are diametrically opposed to the readings of the Hathaway apparatus which I am now willing to discard but which I cannot easily explain. It is evident from these fairly consistent results that we must regard the phenomena of fatigue, when they set in, as of the same direction as that which characterizes both affective and motor responses, namely, a *decrease* in resistance or an increase in counter-potentials from the sweat glands. As far as we now know, then, it would seem likely that

Table V. *Fatigue series with Greenwald apparatus*

	Normal		Fatigued		Difference	
Date	E.D.R.	S.B.P.	E.D.R.	S.B.P.	E.D.R.	S.B.P.
19. ix. 36	63,000	154	48,000	160	−15,000	6
24. ix. 36	55,500	158	73,500	140	18,000	−18
29. ix. 36	70,500	152	37,500	162	−33,000	10
1. x. 36	67,500	158	82,500	162	15,000	4
9. x. 36	85,500	160	81,000	168	− 4,500	8
15. x. 36	82,500	168	67,500	172	−15,000	4
27. x. 36	55,500	158	33,000	168	−22,500	10
3. xi. 36	67,500	160	66,000	166	− 1,500	6
5. xi. 36	64,500	164	51,000	170	−13,500	6
17. xi. 36	51,000	168	27,000	168	−24,000	0
8. xii. 36	43,500	164	31,500	168	−12,000	4
8. iii. 37	118,500	148	36,000	160	−82,500	12
15. iii. 37	88,500	160	72,000	160	−16,500	0
22. iii. 37	94,500	161	58,500	158	−36,000	− 3
5. iv. 37	105,000	151	72,000	151	−33,000	0
*12. iv. 37	105,000	130	51,000	127	−54,000	− 3
19. iv. 37	105,000	160	114,000	163	9,000	3

E.D.R.—electrodermal response in ohms of resistance.

S.B.P.—systolic blood pressure in mm. of mercury column (no calculations or conclusions have thus far been made on these data).

* The author acted as subject for all results except this one.

fatigue symptoms of both the mental and the physical sort would be accompanied by a retarded return of the deflexion to the base-line or, in other words, a delay in the time of recovery.

Before we go into the matter in greater detail we must have many more data from a larger number of individuals. It might also be desirable to separate the type of fatiguing stimuli so that in one series only mental work be exacted and in the other continuous physical work be required. From our long experience this separation would not be as necessary as the logic of the situation would demand because the mental experience from our series is very much the same. It is remarkable in this connexion to what a large extent our introspective records tally with the symptoms

described by Offner: "In consciousness there appear subjective symptoms, like those accompanying bodily work—at first a mood of indifference, then a disinclination to pursue the fatiguing work, together with the desire for a change. We are 'tired' of this work. Then a feeling of languor becomes evident, a feeling that we can't get hold of things, though we still want to. We feel weary for any kind of work (feeling of weariness). Finally, we feel exhausted, and crave nothing but rest and sleep. Then, not infrequently, headache follows—analogously to the soreness of the fatigued muscle; then restlessness and excitement, heightened sensitivity to impressions (hyperesthesia), especially to noises, nervousness, irritability, ill-humour and liability to passionate outbreaks" ((3), p. 16). Typical analytical records have already been published in my recent book ((7), p. 407).

IV. A SUGGESTED HYPOTHESIS

I wish tentatively to advance the hypothesis that possibly fatigue truly represents a retardation of the activity of the higher cerebral centres and a relapse to controls of the lower centres like the hypothalamus. In that event it might be plausible to suppose the same kind of activity to remain that obtains in the affective area through connexions with the sweat glands and the autonomic nervous system.

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THE INFLUENCE OF SOUND UPON ACHROMATIC CONTRAST

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I. *Introduction* (p. 219).
II. *Method of experiment* (pp. 219–221).
III. *Results* (pp. 221–223).
References (p. 223).

I. Introduction

S. V. Kravkov has investigated the influence of indirect stimuli upon the irradiation of light in the eye (1,2). This influence is not always of the same character. It may be such as to produce either an increase of the irradiation of white light (the positive irradiation) or a diminution of it. The former was observed when there was strong contrast between the object and its background; the latter when there was weak contrast.

It was this regularity that caused Kravkov to state the hypothesis that stimulation caused by indirect stimuli is distributed unevenly in the centres affected by the direct stimuli (2). And this stimulation is more uneven when there is a greater difference between the levels of the excitation of the adjacent, directly stimulated fields. This hypothesis leads us to believe that strong achromatic contrast increases under the influence of the indirect excitation; whereas weak contrast decreases under the influence of the same indirect stimulus. To verify the above deduction was the object of the experiments described below.

II. Method of experiment

Three paper disks were fixed to the axis of a small motor (colour mixer). Two of them, one black and the other white, had a greater diameter than the third, which was white. A black arc was placed upon the small white disk. The angle of this arc in one series of experiments equalled 180°. A dark ring, appearing on the white disk when the motor revolved, was under these conditions considerably darker than the white disk (a case of 'strong contrast'). In another series of experiments, the angle of the

black arc equalled 30°. When the motor revolved under the same conditions a light grey ring was seen, which could but slightly be distinguished from the background (a case of 'weak contrast'). In both cases, the colour of the ring against the white background changed under the contrast action of this same white background on which it was seen. The task of the subject was to match the colour of this narrow grey ring, drawn on the white background, with the colour of the big outside disk. The latter, as is pointed out above, was made up of the white and grey sectors whose size could be regulated by the experimenter.

The rotating disks were seen monocularly by the subject, through a blackened tube. Through it the subject saw only the field traced by the

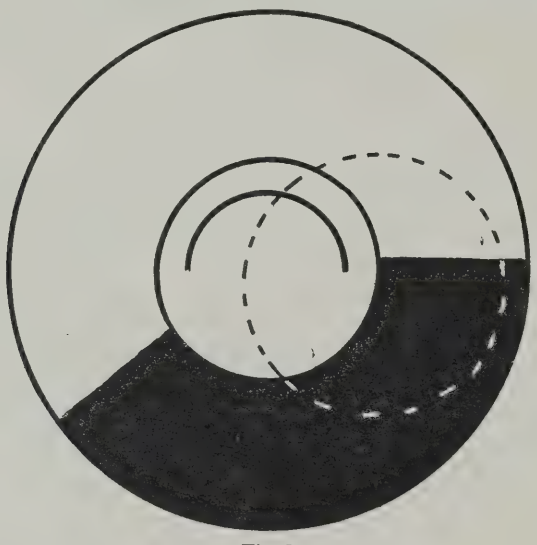

Fig. 1.

dotted line in Fig. 1. This field was seen through the tube at an angle of 2° (the eye of the subject was at a distance of 65 cm. from the colour mixer).

The indirect stimulus was the sound of a valve oscillator giving a frequency of 775 c.p.s. and an average loudness of about 70–75 decibels above threshold. The sound was transmitted to both ears of the observer by telephone receivers.

At the beginning of the experiment the subject was told the following: "There are two disks: a white one representing the 'field' and a grey one representing the 'background'. A grey ring appears on the white disk. Look very closely at the colour of that ring and at that of the background.

When after some time I cover the aperture of the tube, you are to tell me if the colour of the ring is identical with the colour of the 'background'; if, however, it is not, then you must tell me whether it is necessary to darken or lighten the background so that its colour may match that of the ring. Please answer in as few words as possible. Does it match, must it be darkened or lightened?"

The experiment was performed under conditions of light adaptation given by two electric bulbs in a room with white walls. The illumination of the disks on the colour mixer equalled approximately 320 lux. At the beginning of the test the subject fixated a black screen through the tube. As soon as the subject placed his head on the head-rest, the motor was turned on and the black screen was removed. The exposure lasted for 20 sec., and afterwards the disks were again screened from the subject's eye and the colour of the background changed in accordance with the subject's indications. This continued until the subject judged that the colours of the ring and background matched, when the size of the white sector on the greater disk was measured and recorded. This was repeated several times. During the intervals between exposures the subject's gaze was fixed upon the white wall of the room.

After the subject had made four matches, there was a 10 min. interval, which was followed by the sound stimulus. The experiment began after 1 min. of adaptation to the sound. The procedure was exactly the same as with the previous exposures. The subject was exposed to the sound for 8–15 min. (depending upon the speed with which he matched the colours). The sound was then stopped and the subject again given a 10 min. rest period, for the after-effect of the sound to pass off before a final control experiment without sound. The values for the white and black sectors of the greater disk were averaged for the two stages. These averages made it possible to judge the change in the value of the perceived achromatic contrast under the influence of indirect stimulation.

In all there were eighty experiments, not counting the preliminary trial tests. There were fifty-six in the first series, where the black arc on the white background equalled 180°; and twenty-four in the second series where the black arc was equal to 30°. The experiments were performed upon nine subjects.

III. Results

In fourteen cases the sound produced no effect at all. In two cases the effect proved of quite an opposite character, contrary to our expectations. In the rest (sixty-four cases) the result corroborated the hypothesis we

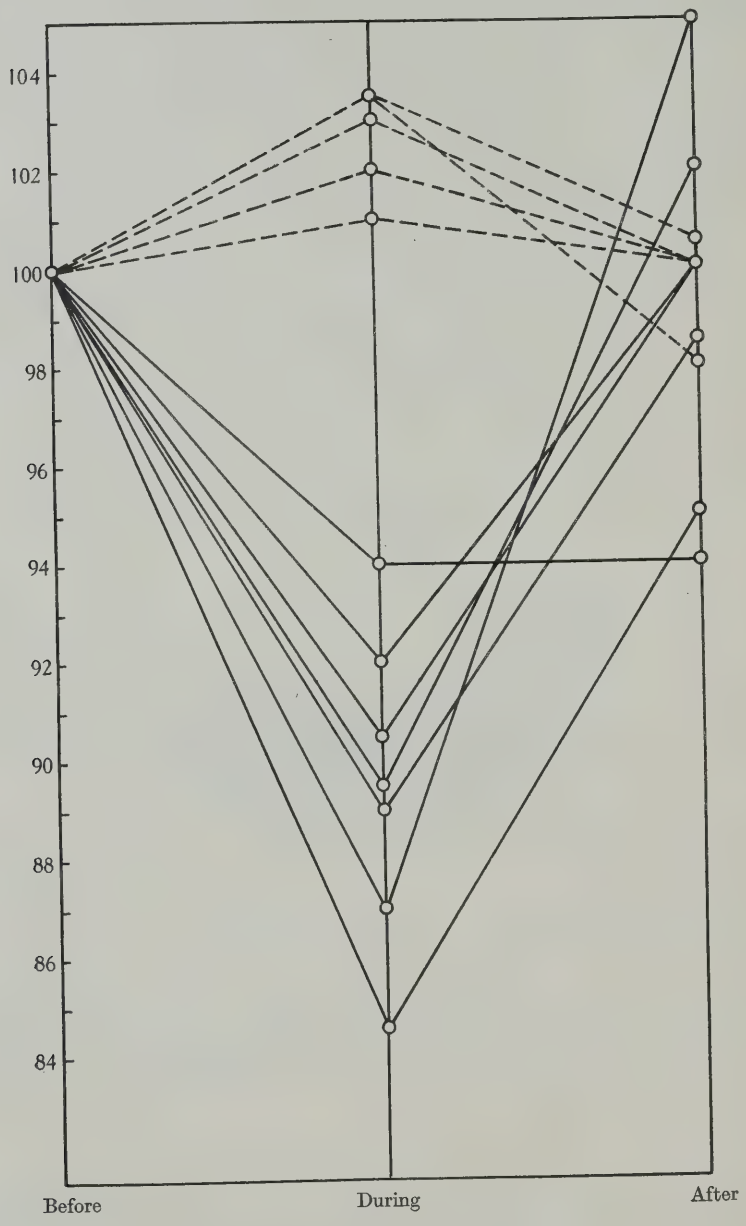

Fig. 2.

set out to verify: i.e. *strong contrast increases under the influence of sound while weak contrast diminishes.*

The average results obtained from all the experiments with all the subjects are given in Fig. 2. The results of the experiments with strong contrast are shown by full lines; those of experiments with weak contrast by dotted lines. The brightness values of the grey of the big disks which the subject considered identical with that of the grey ring on the white disk are marked (in relative values) on the ordinate. The time of measurement 'before the sound', 'during the sound', and 'after the sound' is given on the base line.

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NOTE. Owing to the international situation, it has been impossible to submit the proof of this paper to the author for correction, and for approval of certain slight emendations of the original text.—Editor, *British Journal of Psychology.*

A NOTE ON TACTUAL PERCEPTION IN A BLIND SUBJECT

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I. *Problem* (pp. 224–225).
II. *Method* (p. 225).
III. *Subjects* (p. 225).
IV. *Results* (pp. 226–228)
(1) *Determination of the tactual straight edge* (p. 226).
(2) *Differential threshold for straightness* (p. 227).
(3) *Qualitative aspects of response* (pp. 227–228).
V. *Summary and conclusions* (pp. 228–229).
References (p. 229).

I. Problem

Rubin, in his "Haptische Untersuchungen" (1936), was able to show that the basic dimensions of tactual (haptic) space deviate in certain important respects from those of the visual field. Thus he found that an edge which when tactually explored in a specified manner gives rise to the experience of a straight line is in fact curved in such a way that its centre lies farther from the subject than the two ends. In Rubin's terminology, the haptic horizontal is given by a *plus curved* line ((1936), pp. 288, 290–3, 375).

The degree of plus curvature necessary to provoke an impression of straightness in any given case depends upon a number of factors. One of these, and perhaps the most important, is the axis of the moving member. Thus when the forearm alone is moved, the degree of plus curvature under Rubin's experimental conditions is always greater than when the entire arm is moved about the shoulder as centre (*op. cit.* pp. 292, 375). But with both types of movement the degree of plus curvature which is found to correspond with tactual straightness is always less than that of an arc having as its centre the axis of the moving member. There is thus a sense in which we may speak of *phenomenal regression* (Thouless, 1931) in the tactual sphere.

We have recently endeavoured to repeat Rubin's basic experiment in the Cambridge Psychological Laboratory. But we wished also to obtain some indication as to the responses given by blind subjects under Rubin's

experimental conditions. Hitherto, we have been able to work with but a single blind subject, but the results obtained are not without interest. We venture in the present paper to communicate these results in a comparative manner.

II. Method

The apparatus used was identical in all respects with that devised by Rubin (*op. cit.* pp. 287–8). The methods which we used conformed exactly to Rubin's own procedure. In the first or *short-arm* (s.a.) method, the subject sat at a table opposite the apparatus in such a position that he was able to rest his right elbow comfortably on the table and to extend his hand to touch the outer edge of the steel ruler with the tips of the index and middle fingers (cf. Rubin, *op. cit.* pp. 287–9). The arm was thus allowed to move only from the elbow. In the second or *long-arm* (l.a.) method, the subject sat slightly farther back with the arm extended in a stretched position and the fingers touching the edge as before. In this position, all movement took place from the shoulder. The subject was instructed to try to maintain a light constant pressure of the finger-tips against the outer edge of the ruler and to move them along it smoothly at an approximately constant speed. At the beginning of each trial the subject placed his fingers at the right-hand edge of the bar and moved them across and back twice. He was then required to give a judgement of *plus curved*, *straight*, or *minus curved*. The constant method was adopted throughout in accordance with Rubin's procedure. No indication was given to the subject at any point as to the correctness of his judgements, and in the case of the normal subjects, the apparatus was of course concealed from view by an arrangement of screens.

III. Subjects

Four subjects co-operated in the experiment. One of the subjects, W. L., had been totally blind for some twenty years. The remaining three were undergraduates with normal vision. Two of these three subjects, B. L. and J. S., were women, and the third, A. St., a man. The average age of the three undergraduate subjects was twenty-one years.

Subject W. L. was a very highly educated woman and most willing to co-operate in the experiment. Her history is briefly as follows: Before the age of about six years, the subject remembers being just able to appreciate forms at close range (e.g. a sixpence held before the eyes). From six to approximately fifteen years of age, she could react only to light, shade and colour. The appreciation of form was no longer possible. For the last twenty years the subject has been totally blind.

IV. Results

(1) *Determination of the tactual straight edge*

The degree of curvature corresponding to a tactual impression of straightness (*haptische Geradheit*) for all four subjects under the two types of condition is given in Table I (columns 1 and 3).

Table I

	L.A.		S.A.	
Subject	Determination of straight in mm.	Difference threshold in mm.	Determination of straight in mm.	Difference threshold in mm.
B. L.	+2·5	1·5	+3·0	3·5
A. St.	−3·0	2·0	+4·0	2·5
J. S.	+5·0	2·0	+9·0	2·5
W. L.	+5·0	2·0	+4·0	2·0

The table brings out the following points:

Firstly, in every case but one the subjective determination of *straight* is a plus curve;[1] that is to say, one in which the ends of the ruler are bent towards the percipient. The actual values show considerable individual differences, but they are all of the order found by Rubin in his original study. It will be observed further that the two values for the blind subject are distinctly on the large side, being exceeded only by subject J. S. in our investigation and by one of Rubin's original eight subjects (cf. Rubin, *op. cit.* p. 292, Tabelle 2).

Secondly, it will be noted that with the solitary exception of the blind subject, *haptische Geradheit* is in fact more curved under S.A. conditions than under L.A. conditions. This result, too, is in good agreement with the findings of Rubin (*op. cit.* pp. 292, 375). At present, however, we do not see our way to accounting for the discrepancy between the results for W. L. and those obtained from the remaining subjects. It may be that some special constancy mechanism is developed in the blind, but further empirical investigation is obviously demanded before any hypothesis of this nature be seriously put forward.

[1] This solitary instance of an experience of phenomenal straightness being given by a *minus* curve is probably to be explained largely in terms of an intentional process of compensation. This subject (A. St.), who throughout adopted an extremely critical and 'object directed' attitude, remarked during the session in which this series of readings was taken that he assumed that anything which seemed to coincide with the arc of a circle made by his hand must be a plus curve and that he therefore tried to compensate accordingly. A not dissimilar case of minus curvature is reported by Rubin (*op. cit.* pp. 292–3).

(2) *Differential threshold for straightness*

An approximate measure of the differential threshold (*Schwellenwert*) for tactual straightness (as differentiated from curvature) is also given in Table I (columns 2 and 4). It is interesting to note that the differential threshold of the blind subject averages 2·0 mm., which is slightly lower than that of the remaining three subjects, whose average values are 2·5, 2·25 and 2·25 mm. respectively. This comparatively low value of the differential threshold in the case of W. L. does, it is true, point towards a slightly higher consistency of judgement, but the lack of statistically significant differences between the individual results precludes our drawing any conclusions bearing upon the much disputed questions of 'tactile acuity' in the blind. It may be added that the high consistency of judgement shown by W. L. certainly does not correlate with objective accuracy; her average determinations of *straight* are 1·75 and 4·0 mm. greater than the corresponding values obtained from subjects B. L. and A. St. respectively.

(3) *Qualitative aspects of response*

Individual differences in general attitude towards the experimental task were very striking. Two of the four subjects, namely B. L. and A. St., adopted highly critical and analytic attitudes on every trial. These two subjects, it will be remembered, showed a relatively small degree of plus curvature in their determinations of the straight edge. Subjects J. S. and W. L., on the other hand, were far less 'object directed' in general *Einstellung*, and both experienced considerable difficulty in responding to the presented edge as a single, isolated stimulus object. This was particularly the case with the blind subject, who remained surprisingly *naïve* throughout the experiment. These findings appear to indicate that the adoption of a critical attitude favours phenomenal regression to the 'real' character of a tactually presented object.

A second finding of some interest was that the judgements given by the blind subject were apparently determined in an overwhelmingly large number of instances by factors of the order of 'absolute impressions'. She made far less frequent use of 'side comparisons' than was the case with the three control subjects. These features comprised the most salient points of difference between her introspective reports and those of the normal subjects.

The blind subject claimed also that she apprehended what she called tactile 'motion' with the utmost facility. By this, she appeared to mean that she became aware of a change in the curvature of the Rubin edge in

much the same manner as she normally appreciates a change in the direction of a vehicle in which she is travelling. In both cases the perceptual interpretation is so rapid as to manifest itself as a virtual 'intuition'. This observation, though no more than an analogy, does indicate that tactile and labyrinthine 'cues' may come to be interpreted by the blind with the characteristic immediacy which we associate with visual apprehension. These questions would seem to suggest an interesting field for future investigation.

V. Summary and conclusions

(1) A repetition of Rubin's experiment on the determination of *haptische Geradheit* was undertaken. The subjects comprised three undergraduates with normal vision and a woman who had been totally blind from an early age.

(2) The results show firstly that the experience of *haptische Geradheit* is typically given by a *plus curved* edge; secondly, that the degree of plus curvature is, as a rule, greater under 'short-arm' (S.A.) than under 'long-arm' (L.A.) conditions of tactual exploration. These results are in good agreement with the findings of Rubin.

(3) The values of plus curvature in the case of the blind subject are distinctly on the large side, being exceeded by only one subject in our investigation and by one subject among Rubin's original eight observers.

(4) There is a reversal of the typical relations between the values obtained under L.A. and S.A. conditions in the case of the blind subject. For this subject alone, *haptische Geradheit* is more curved under L.A. than under S.A. conditions. This finding has no parallel either in Rubin's results or in our own. It is, however, pointed out that further experimental work is required before any conclusions can be drawn.

(5) The average differential threshold for *Geradheit* for the blind subject is slightly lower than the values obtained for the remaining three subjects. These differences are not, however, statistically significant.

(6) Individual differences in general attitude were very striking, and it was found that the adoption of a critical *Einstellung* appeared to favour phenomenal regression to the 'real' character of the tactually presented edge. The blind subject was extremely *naïve* in her attitude towards the experimental situation, and made much use of 'absolute impressions' in effecting her judgements. Certain other features emerging from a consideration of the introspective reports are discussed.

(7) The Rubin edge would seem to offer a useful tool in the investigation of certain perceptual problems presented by the blind. We believe

that further work along the lines indicated in this preliminary study would furnish results of some interest from the standpoint of general psychological theory.

We have to thank Mr K. J. W. Craik for constructing a replica of the Rubin apparatus, and Mr E. G. Chambers for some advice on a statistical treatment of our results.

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TWO CONTRIBUTIONS TO THE THEORY OF MENTAL TESTING

II. A STATISTICAL REGARD OF PERFORMANCE

By W. STEPHENSON

From the Institute of Experimental Psychology, Oxford

I. *Introduction* (p. 230).
II. *Thesis* (pp. 231–232).
III. *The bases of quantification* (pp. 232–237).
IV. *Response items and method of appraisal for the new performance test* (pp. 237–239).
V. *Illustrative example* (pp. 239–242).
VI. *Factor analysis* (pp. 242–245).
VII. *Conclusion* (pp. 245–246).
References (p. 246).
Appendix (p. 247).

I. Introduction

The present paper continues the previous one in which a new performance test was described (1), and like that paper the present one is a contribution to the theory of mental testing. Both papers have as their concern *Q*-technique, the use of *persons* as variables, which I seek to distinguish from *r*-technique, the use of *tests*, etc., as variables. It is my contention that these two techniques should not be regarded as merely alternative ways of treating one and the same matrix of data (8); on the contrary each offers its own characteristic approach to psychometry. *Q*-technique can have its own special problems, its own assumptions, principles and objectives, and these can be independent of those involved in *r*-technique. In the previous paper I offered a demonstration of this by considering some of the conditions under which *ability* might be measured directly as a correlation coefficient, and in the present paper I propose to give another demonstration of the unique part that correlation of persons can play in mental test theory, by showing how persons can be correlated and factorized for the *quality* of their work at a performance test. I seek to reduce performance, in all its qualitative richness, to the neat precision of a mere correlation coefficient.

II. THESIS

Our concern is to be with what the trained psychologist can observe and appraise as he overlooks an individual at work on a performance test. The setting is to be the performance test already described, but the matters to be raised are of general theoretical interest.

Mental testers are often expert at describing the main temperamental and other personality characteristics of the individuals they test, and indeed individual testing is now used with an eye to this wider clinical diagnosis rather than with the exclusive object of supplying a measurement of a person's I.Q. (2). It is now often said that the I.Q. measurement is the least important of the facts that issue from testing a person. At present, however, there is no systematic way of dealing with this wider diagnosis; the Binet has been subjected to some such treatment (3), and the Rorschach test has claims to consideration in this connexion (4), but for the main part the psychologist's observations are of an incidental nature, which he describes in the form of 'case notes'. It is in no way suggested that these literary productions should be replaced by statistical treatment of the kind to be considered in the sequel, but it seems worth while asking whether a method can be devised which will take due account of these incidental observations and at the same time make them amenable to comparative and systematic study.

The psychologist may direct his attention to the subject's general demeanour, his dress, attitudes and the like, or to the intimate details of the performance itself. Much will be learnt about the individual's personality from both these sources, which are far from unrelated, for even the colour of a subject's hair may help us to appreciate why he swoops on the performance pieces like a hawk or dilly-dallies with them like a cat playing with a mouse. But for the test to have any special claim to attention it must supply its own quota of information about the person tested, information which depends intrinsically upon the test material itself. In the case of a performance test this has hitherto usually consisted of a bald statement about the probable I.Q. of the person.

When an I.Q. is measured, little direct account can be taken of the details of a person's performance. If it is a *timed* performance test that supplies the measurement, the individual may work over-cautiously, slowly, and with lack of foresight or system—indeed his work may involve a thousand and one qualitative characteristics of this kind; and yet all that the I.Q. is directly concerned with is the time it takes him to arrive at the required solution. This does not mean, however, that these quali-

tative characteristics are completely overlooked in the time score, for clearly at least some must vary in direct proportion to the time it takes the individual to solve a task. He cannot work inattentively, unsoundly, and with lack of judgement, and at the same time solve the task quickly; and the person who solves it slowly cannot usually have done so aptly, and with efficient constructions. The time measure thus embraces some of the qualitative aspects of performance. The trouble is that we do not know which, and much is taken for granted that should be explicitly attested, and important qualities such as perseverance, initiative and the like might be very unsatisfactorily accounted for in the time measures. Clearly these myriad qualities of performance, in all their diversity and complexity, should be investigated on their own account, if possible *in situ*. It is well known that performance tests afford an excellent medium for laying bare these qualitative characteristics. One can almost see what the person is thinking about by looking at his finger tips.

The thesis that I am to develop in the sequel is that performance, smooth-flowing stream though it may be, can be regarded as a population of innumerable response items or qualities of response. These, with all their interconnexions, can be evaluated for their relative *significance*, or importance, or dominance, in the individual's work as a whole. It is thus permissible to select a sample of these items and to quantify them item by item for their relative importance in the individual's total behaviour, and thereupon to correlate one person with another for a given sample of these response items. Two persons performing a test with the same kind of qualitative picture, therefore, would correlate positively. A number of persons could be intercorrelated, and factorized. The implications of such a thesis and technique, and the purposes to be served by this factor analysis, are considered in the sequel.

III. The bases of quantification

All the qualities of response that appear in the course of work at performance tests, such as foresight, perseverance, initiative, trial-and-error thinking, confusion, shape dominance, imaginativeness, motor agility and the like, can be quantified in at least two distinct ways which are methodologically as different as chalk and cheese. In one case the qualities may be quantified in their *immediate* context, which is the concern of *Q*-technique, and in the other the qualities may be considered in more *general* contexts, each in its own right as a possible ability, and in this case *r*-technique is involved. In the former case a basis of

quantification, in terms of which different qualities may be compared, is the *significance* of each quality in the particular performance, or kind of performance. The concept of *significance* is the major theme in the present paper; it is the outcome of special needs, is peculiar to *Q*-technique, and as such the concept I think may be considered to be new to systematic psychometry, although something of the kind has long been implicit in any clinical regard of mental testing.

It has already been stressed, in the previous paper, that the premisses and assumptions which underlie our testing practices and theories are apt to be overlooked, and it is necessary to attend to these for the purpose of distinguishing between *Q*- and *r*-techniques. Thus, in *r*-technique each quality is a variable, and quantification pertains to a statistical population of persons, assessed or measured *for amounts of that particular quality*. If our concern is a quality such as perseverance, to take this as an example, we appraise or measure each person for his degree of that quality, and usually there are ideas at the back of our minds about the kind of frequency distribution to be assumed or expected. Our essential interest is with perseverance in a general way: in particular cases the individual may be bored; exhausted, or discouraged, and show little perseverance; but *r*-technique is only concerned with his average or more general *capacity* for that quality. When we test him for this quality we try to ensure that he will have every chance to do justice to himself; we make the conditions propitious, and remove any militating influences. And we would seek to make any tests of perseverance as free as possible from contamination with other qualities. That is, whether we merely estimate the quantities or objectively measure them, the assessments are based on the premiss that the conditions for all individuals are equal or controlled with respect to all other qualities except the one under consideration at the moment. Every quality, as far as possible, is quantified with respect to controlled conditions, or else, what is the same thing in effect, the quantities are average or general amounts.

Quantification in *r*-technique, then, involves a separation of the qualities from any particular contexts; each quality is separately quantified under controlled conditions, or is an average of innumerable events. A complete sample of individuals, one by one, may be quantified in this way for any number of different qualities—for perseverance, synthetic thinking, confusion, and the like. Finally the arrays for these different qualities may be correlated and factorized, with the object of seeing whether the quantities for the different qualities vary proportionately, or display some kind of orderliness or system. By far the greatest part of

current mental testing is founded on quantification and conceptions of the above kind—all the factors of Spearman, Thurstone and Kelley have their origins in *r*-technique, as have the more empirical formulations of a Binet, Terman, or Rorschach.

The many qualities that we can observe in an individual's work at a performance test are quantified, in *Q*-technique, under very different conditions. We now attend to more *particular* events, to the work of one person only, if need be, in such a test; or we might concern ourselves with his work in a particular kind of performance test, such as that constituted by the twenty tasks of the new performance test described in the previous paper (1). The twenty tasks of that test series are very similar in material and form, and it is possible to focus attention on the individual's work in all twenty, on the ground that essentially the same performance is mirrored in each. In *Q*-technique we deal with the person as variable, and the many qualitative features of his work have to constitute a sample that has been drawn from a statistical population of such qualities pertaining to the individual's work. Moreover, a common basis has to be found for mutually comparing these qualities *in situ*, in their particular context, just as they appear in an individual's performance. The basis might be qualitatively different for each person, but the many different qualities or responses that comprise one person's work must be quantified on a common basis relative to each other; we are indeed to contemplate the individual differences *of these qualities* in a person's performance, for some characteristic they all have in common.

We should ask, first of all, in what sense a number of responses and characteristics of work at a performance test can be said to constitute a statistical population of qualities. Certainly countless qualities may be involved, but they are connected in a complex and ever-shifting causal and logical system. Performance shows main lines of endeavour and direction towards ends. It is an irreversible stream of behaviour, flowing with more or less directness into the final sea of all tasks that are ended and done with. Yet all this flow of cause and effect, of stimuli and response, has to be conceived of as a mere statistical heap of qualities, a jumble of unrelated units! But, as I tried to show in the previous paper for similar doubts about the conditions under which *tests* could be said to constitute a statistical population, the units that we deal with in psychology need not be conceived of as unrelated, so much as so complexly and completely interconnected, so highly ramified in their interrelations, that only a statistical regard can do justice to them. When the relations between the items are so innumerable that the theory of 'errors' can be

applied to their quantities, the statistical regard is not without justification. It will be admitted that my chest measurement is not less real for regarding it as dependent upon innumerable small influences, depending upon heredity, exercise, food, habits and the like(5). No less tangible is the stream of behaviour in performance when I regard it for convenience as a statistical universe of items, from which I can draw samples.

New brooms, however, should be fitted with new handles, and performance tests can be made which are specially fitted to this statistical regard of performance. They can entice the individual into displaying a rich variety of behaviour; and special procedures can be devised, new forms of tests, which will make it less and less unreasonable to regard the qualities of a person's work as items in a statistical population. In the sequel, for example, I appraise the work of an individual in twenty comparable tasks, and in this way achieve something that is at least a pale reflexion of the required conditions.

As for the common basis of quantification, I put forward the concept of *significance*. The different qualities of a person's work can be quantified with respect to their relative significance in the test situation they comprise. Some characteristics of a performance are more important than others; they lead rapidly to a solution, or bring the individual up against a blank wall and stop his progress. Others seem to colour the whole work, and are by far the most significant responses in the situation—as, for instance, when the individual is in a dither of indecision, or when he suffers from temporary *aboulia*. Significance is in no essential way dependent upon the *frequency* with which a response may appear. We can readily count whether one characteristic appears more often than another, and some undoubtedly are significant because they occur repeatedly in a test performance; but the most significant response item or quality in a performance may make its appearance for an instant only, and on one occasion only. A flash of insight, for example, might be of this nature. The significance of a quality, then, is far more than frequency of occurrence. It is an evaluation, made in the light of the total test situation, and relative to that situation, of the immediate appearances and their underlying causes and consequences. The concept requires, no doubt, further elaboration and illustration, but that can be attended to later. Meanwhile a question remains, concerning the *units* of quantity. We might assume, to make a beginning, that the quantities for a large sample of qualities that have been appraised for their significance, are distributed on a 'cocked hat' frequency curve. Only a few items will be very

significant in a performance, and only a few extremely insignificant: most will be intermediate in amount.

Again, however, technical resources can help us, and this last assumption is not really needed. Nor does it do justice to the facts, for significance can have a positive and a negative direction, and *most* qualities would seem to be of little significance in a performance. The technique I have to describe in the sequel makes it unnecessary to postulate an arbitrary frequency distribution of quantities—it again owes its efficacy to the use of twenty comparable, approximately equal, tasks. Quantities are scaled from positive values for the qualities that are significant in helping the performance towards its proper objective solution or end, to negative ones that are no less significant, but in blocking and delaying the solution. Most qualities are usually intermediate: they neither help the solution forward very much, nor hinder it.

Finally, upon whom or what are we to depend for the quantification of qualities of significance? I can think of no test instrument, other than the psychologist himself, which could render an account of the required quantities. No instrument or test can tell the watchmaker that the balance wheel is the most significant part of a watch; and no tools or tests can tell the physicist that the characteristics of a valve are the most important regulators of a wireless set. Knowledge of this kind is technical, part and parcel of the craft or science involved, and it comes only from the craftsmen or scientists concerned. Similarly in the case of a performance test, only the psychologist can decide upon matters of significance. This by no means implies that appraisal for significance is merely a subjective matter, for any one psychologist's appraisal can be checked against the appraisals of others. Moreover, in so far as the onlooking psychologist draws upon his technical knowledge, his estimations can be as expert and as scientific as the main body of information upon which he has formed his judgements. He merely mirrors the science of which he is a member. With due safeguards, common in any science, I see no reason why we should not depend upon the psychologist for these appraisals of qualities for the significance in a test situation. We grant the judge his expert eye for the fine points of a horse; no less a respectability should be granted to the clinical psychologist with an expert eye for the finer points of a test performance.

The stage is now set, then, for an illustration of this application of *Q*-technique to performance. There is to be a frank acceptance of the psychologist's role as onlooker. I myself will act as observer in the example to be described, and my appraisals will be governed by the kind of

systematic psychology that I think is fairly current amongst mental testers. Attention is to be paid to responses which are intrinsic to test performance. There is to be a rich array of performance to observe, the responses and qualities of which can be regarded as a statistical population. Any sample drawn from this population of response items is to be quantified with respect to the relative significance of each of its items in the performance as a whole. And for several reasons, amongst them considerations of reliability and validity, a large number of comparable tasks are to be evaluated. Using the sample of qualities as a population, and a number of persons as variables, correlations are to be calculated and factorized. The objects to be served in this way are to be referred to in the sequel.

IV. RESPONSE ITEMS AND METHOD OF APPRAISAL FOR THE NEW PERFORMANCE TEST

The above theoretical conceptions find a ready practical illustration in terms of the new performance test (1). It will be recalled that the test consists of twenty tasks, each involving a number of pieces which have to be placed in juxtaposition so as to make a shape which, as nearly as possible, is a match of the given standard shape. I propose making use of data gathered from the forty students already referred to in that paper, and it will be remembered that I use these students purely for illustrative purposes, for the appraisals could as readily have been made for any subjects capable of handling the test material.

As a result of preliminary observation of students I drew up a list of response items and qualities that I could readily observe, or infer from observation, whilst an individual performed these tasks. From this *ad hoc* list I selected sixty at random; this sample is given in the Appendix. Each quality was carefully defined beforehand, and only the short titles are given in the Appendix. No. 1 item, for example, was defined as follows:

> Tendency to shift pieces from one position to another without due cause; without looking at pieces they are picked up and then vaguely put down again; tendency to grasp impulsively at pieces, and to replace them randomly.

It was expected that I keep these meanings in mind and report unvaryingly upon them, but I could at any time refresh my memory by referring to the full list of definitions—which I always did prior to beginning an appraisal. The sixty short titles were cyclostyled, so that a clean copy was available for appraising each individual. The items are merely a sample of the many responses that can be observed or inferred in

the course of an individual's performance, and each could be subdivided in most cases, if need be, so further swelling the total population to be considered. But a beginning has to be made somewhere, and this sixty is quite adequate for the present purpose.

It is important to notice that the twenty tasks (A to T of the previous paper) are very similar, involving similar problems, goals, and material, and making similar demands upon the thinking and manipulative abilities of the individual. It is as though the same task were presented to him, in a modified form, on each of twenty occasions. For this reason the sample population of responses can apply to each of the tasks in turn, or to all twenty as a whole. This is of considerable practical assistance, because it is possible to appraise each task in turn and to sum the results for all twenty, thereafter using this summation as the required evaluation of the items for their relative significance in the individual's performance. This not only makes for reliability and validity, but is indeed a means of arriving at reasonable frequency distributions for each person, and also gives an appraisal that more truly represents the kind of performance involved in the test as a whole. Our concern is still with a particular event, but we repeat it so as to appraise it the better.

With this in mind it is most convenient to appraise an individual's work in the following standard manner: As the subject works at the test the observer sits in good view of all the performance, and goes through the cyclostyled list of items, one by one, placing a *tick* against those which are significantly displayed in the performance, and a *stroke* if they are conspicuously absent in conditions otherwise calling for their display or if they are otherwise negatively significant. Two ticks or strokes might be given to a task on occasion, if the quality concerned seemed to be extremely significant. The practice is followed of first observing the individual's work for half a minute or so, and then beginning the appraisal. At the conclusion of the twenty tasks a count is taken of the number of ticks or strokes (or difference between them) adjacent to each response item, and this number is thereafter regarded as the score for significance of that item, for the individual concerned. It is important to notice, as I indicate later, that these scores need bear no relation to a person's capacity or ability for the qualities in general. Items which are not conspicuous in either direction, or which are neutral, tend to have zero significance. It is not that these items are absent in the person's performance, for by assumption and fact they are present—they appear in some or most of the twenty tasks, if not in all. They are merely responses which are scarcely of moment in the task, of little apparent

consequence in it. A response item which is neutral for nearly everyone, can be highly significant in another's performance. Thus indecision, which is of no great moment in the work of most subjects, can on occasion completely colour a person's work, being by far the most significant quality of it. It is usually not difficult to decide when a quality is significant in a performance, either positively or negatively, because it marks a step forward towards a solution, or markedly delays or blocks that step forward. Indecision, confusion, excessive timidity, trial-and-error procedure, extreme competency, and the rest of the sixty qualities, any of these can colour every other with its own character, or stand out above them as the most striking and obvious feature of the individual's work.

V. ILLUSTRATIVE EXAMPLE

Data were gathered from the forty students already alluded to, using the above technique, all appraisals being made by myself. The facts to be considered consist of an array of scores for each student, each array consisting of the summed scores (ticks or strokes) given to each of the sixty response items or qualities listed in the Appendix. Consideration of these data should afford a good example of the whole matter discussed above.

Following each quality in the Appendix, I give the scores for significance obtained by each item as the result of appraising *one* student's work. This is one of the forty arrays we have to consider. Each student's array is very similar, all having a mean of approximately nought, a standard deviation of about 4 units, scores ranging from about $+12$ to -12 in each case, with a few students of very colourful performance having a range greater than this, from about $+16$ to -18.

Table I may be of interest. It gives the grand distribution of scores obtained by the sixty items, for all forty students. There are 2400 appraisals in all, the mean score being $0{\cdot}528 \pm 0{\cdot}097$ (S.E.), and standard deviation $4{\cdot}74 \pm 0{\cdot}068$ (S.E.), the distribution having the familiar 'cocked hat' shape. There has apparently been a tendency for me to give relatively fewer scores adjacent to zero (-1, 0 and $+1$) than might have been expected, as is shown by modes at $+2$ and -3; this means that when I have been in doubt about the significance of a quality I have tended to give it a mark (tick or stroke) rather than not.

The simplest way of dealing with these arrays of scores is to correlate them, the variables being students, and the population common to these being the sixty response items or qualities. A typical set of intercorrela-

Table I. *Distribution of scores given to the sixty response qualities, for all forty students*

Score (ticks)	No. of times this score is given to qualities	Score (strokes)	No. of times this score is given to qualities
16	4	− 1	138
15	3	− 2	182
14	2	− 3	230
13	1	− 4	155
12	12	− 5	106
11	19	− 6	54
10	40	− 7	49
9	51	− 8	21
8	27	− 9	26
7	48	−10	12
6	95	−11	4
5	151	−12	1
4	170	−13	3
3	219	−14	5
2	224	−15	.
1	158	−16	1
0	177	−17	7
		−18	5
		Total	2400
		Mean	0·528
		S.D.	4·74

tions for sixteen of the students is given in Table II. I have not considered it necessary to provide the intercorrelations for all forty students—a total of 780 correlations in all—because Table II is quite sufficient to illustrate the way in which all such data can be treated.

The correlation table has been arranged so as to bring out immediately the similarities and differences between the students. It requires no knowledge about factor methods to note that the students fall into three groups, students 1–4 into group I, 5–12 into group II, and 13–16 into group III, the students of one group correlating more amongst themselves than with students of the other groups. Positive correlation between two persons means that their performances are *qualitatively* similar, and the higher the correlation the greater the similarity. Negative correlation means that their performances have opposite qualities to the fore: what is positively significant for the one person is negatively for the other. If two persons do not correlate significantly their performances bear no qualitative resemblance to each other; they involve the same qualities, but their significance in the one person's performance is random compared with that of the other person's. The correlations in Table II show that the larger proportion of the students (nos. 5–16) intercorrelate positively, and to the extent of the correlations shown their work must

Table II. *Showing product-moment correlations (Q) for sixteen persons (1–16) for a population of sixty response items. The persons are arranged in order of types, I, II and III respectively.*

	Persons	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Type I	1	—	380	466	224	060	069	309	−011	073	−121	201	035	−550	−380	−240	−314
	2		—	335	316	069	081	−050	106	080	−002	183	118	−462	−551	−600	−518
	3			—	291	−160	022	051	−003	−084	−031	008	053	−334	−427	−511	−444
	4				—	090	015	−033	−065	033	−095	064	075	−221	−317	−440	−392
Type II	5					—	490	523	432	471	416	209	111	−082	314	250	−093
	6						—	300	267	284	298	141	233	018	202	121	031
	7							—	400	310	327	071	162	070	332	340	125
	8								—	241	331	095	205	071	193	405	088
	9									—	505	274	183	111	093	213	050
	10										—	231	255	311	292	336	214
	11											—	302	108	157	209	063
	12												—	301	277	250	099
Type III	13													—	520	505	493
	14														—	640	564
	15															—	611
	16																—

display qualitative similarities. Students 1–4, on the other hand, whilst correlating positively amongst themselves, correlate negatively with students 13–16. On the average there is very little significant correlation between students in group I and those in group II.

The three groups of students can be studied by averaging the scores given to each response item *for the students of a group*, so arriving at an average array which represents students of that group. The three averaged arrays can then be compared; in particular it will be most interesting to note which of the response items are most significant (positively or negatively) in the three groups, and which items show the biggest difference in significance in one array compared with another. Thus, students in group I were chiefly notable for the following qualities in their performance:

Capable space-sense ability, methodical, deliberate, observant, capable, rapid, logical-minded, makes little use of trial-and-error, uses clues efficiently, no motor disabilities, imaginative use of material, analytical, helpful use of abstraction and generalization, colour clues are used first, not dominant for either shape or form.

Students in group II were notable for the following qualities:

Much use of trial-and-error, synthetical, rapid, practical, medium space-sense (or poor), tendency not to use colour clues first, shape dominant, given to impulsive over-moving, logical, accept inexact constructions.

Students in group III gave the following significant picture:

Predominant use of trial-and-error, little dependency upon available clues, little generalization or abstraction, inexact constructions accepted, stiff in movement, unimaginative use of material, medium or poor space-sense, slow, unobservant.

Persons in group I clearly appear to be most capable, and those in group III least.

VI. Factor analysis

The above empirical analysis can be represented precisely in factor form, and of the various factor techniques that might be employed I prefer the following: Persons of group I may be regarded as of a *type*: they satisfy the *theorem* of two factors amongst themselves. Let this type be represented by a factor I. Similarly each of the other two groups can be represented by factors II and III respectively. Table II shows that these factors tend to be *oblique*, that is, they correlate with one another—in particular factor I correlates negatively with III, but is orthogonal with respect to II. I suggest elsewhere (6) that factor representation by, if necessary, oblique factors, is the first and essential purpose of factor study, and that analysis of correlations into fractional orthogonal factors serves a secondary purpose, or merely helps one to arrive at

the required oblique factors. Factors such as I, II and III are *unanalytical*, in the sense that they involve no fractional regard of the available correlation.

The factor-saturations of the students themselves are calculated in terms of students of a type *only*. Thus, considering first the four students of type I (persons 1–4), their factor-saturations are calculated relative to each other by use of the Spearman formula 16A[1], with the results given in Table III (column (i)). Next the eight students of type II have their factor-saturations calculated, relative to themselves only, from the same formula, with the results shown in column (ii). Finally the same treatment is given to the four students of type III, with the results shown in column (iii).

Table III. *Showing oblique factor-saturations for the sixteen persons 1–16, derived from Table II*

	Factor-saturation (Spearman formula 16A)			Factor-saturation (Modified Spearman 16A)[1]		
Persons (1–16)	I (i)	II (ii)	III (iii)	I (iv)	II (v)	III (vi)
1	624				122	−489
2	600				128	−713
3	650				.	−573
4	439				.	−455
5		736		.		.
6		530		076		111
7		550		.		275
8		516		.		232
9		611		.		150
10		644		−096		385
11		328		180		173
12		367		117		305
13			650	−664	192	
14			792	−718	429	
15			805	−761	491	
16			745	−716	121	

But for comparative purposes it is also useful to have the factor-saturations of each person in each of the *other* factors, and these are given in the remaining columns of Table III. They are calculated from a modified form of formula 16A,[1] in which persons of a type are used as 'reference

[1] The well-known formula is

$$r_{ag} = \left\{\frac{A^2 - A'}{T - 2A}\right\}^{\frac{1}{2}}.$$

(From Spearman, C., *The Abilities of Man*, Appendix, p. xvi.)

The modified form is

$$r_{ag} = \left\{\frac{A^2 - A'}{T}\right\}^{\frac{1}{2}},$$

where a is the student whose saturation is required, g is the factor, A is the sum of the correlations between a and the reference value students, A' the sum of the squares of these correlations, and T is the sum of the intercorrelation between the reference value students for a matrix in which both halves are entered.

values'. The factor saturation of a person in any *other* type is obtained by adding him to the matrix of 'reference values' for that type; never more than one person is added at a time, and he is removed from the reference matrix as soon as his saturation has been calculated. A more detailed account of this typology is given in the paper previously mentioned (6), in which I examined Prof. Burt's recent important papers on correlating persons. It may be observed in passing, however, that the types here contemplated are not necessarily unique or distinct—all in fact involve the same qualities, and one type may correlate with another. Table III shows that type II is almos tuncorrelated with the other two; but types I and III are negatively related, as columns (iv) and (vi) show.

Using a short method that I need not go into here it is easy to determine into which type, if any, the rest of the forty students would fall if they were added to Table II. Of the forty it appears that seven belong to type I, ten to type III, and twelve to type II; four might as readily be placed in types I or II, and the rest (seven) equally well in either type II or III.

With a larger population of qualities a finer differentiation into types might be expected; just as with a smaller population the types may be less clearly distinguished—type II, for example, might then be swallowed up into types I and III.

There is little that is surprising in the three types: some are superior performers (type I), others average (type II), and others inferior (type III). As might be expected, there is a tendency for the student to be less significantly capable, less observant, less imaginative, etc., as we pass over from type I to II, and from II to III. The qualities which account most significantly for the differences between the types can be distinguished in detail: thus types II and III tend to be synthetical workers, constructing their copies of the standard shape in terms of what is given on the pieces rather than in terms of explicit analysis of the standard; type I students, on the other hand, take a more analytical approach to the tasks. Type II are shape dominant, whereas type III are either colour dominant or else negligent of colour altogether; type I are dominant for neither, but make use of both colour and shape appropriately.

It is important to notice that persons of type I would undoubtedly fare better than those of type II in performances of a *controlled* kind, in which all alike, for example, are called upon to work synthetically only. Type II individuals, when faced with a task that permits of either a synthetical or an analytical method of work, appear to take the line of least resistance, and work synthetically. But this is merely a preference, and by no means

does it imply that they are the most *capable* synthetical workers. Similarly in the case of colour and shape dominance: under controlled conditions for these qualities type I persons might show themselves to be pronouncedly dominant in one or other direction. That is, the *significance* of a quality in a particular situation need bear little or no direct relation to the controlled measurement of that quality. No better example could be afforded of the difference that exists between *Q*-technique and *r*-technique than this divergence between qualities considered from their *significance* and ability or capacity aspects respectively.

VII. CONCLUSION

It is possible, and I think permissible, to regard a person's work at a performance test as a statistical heap of qualities which can be compared for their relative significance in his performance as a whole. In this way some account is taken of the many details of a performance, in their immediate or particular context.

My objectives have been primarily theoretical, but the above application of *Q*-technique to mental testing can be put to practical use, and might do something to help those who have long drawn attention to the need for systematic study of the qualitative details of performance. When an I.Q. is measured by use of a performance test, the baldness of the measurement could be covered by a detailed array of the qualities that enter into it, or that it conveniently overlooks. This would greatly facilitate the orderly and intimate comparison of individuals. Moreover, it is in no way essential or necessary to work with large numbers of persons, for sound correlational comparisons can be made with as few as a pair of twins, or one or two families of siblings. For work on twin resemblances, or for any detailed comparison of individuals, the statistical view of performance can be invaluable.

On the theoretical side I have just skimmed the surface of a topic that requires very much more analysis and illustration. For the present, however, in the two papers of which this is the second it has been my purpose to demonstrate that *Q*-technique, the use of persons as variables (as distinct from *r*-technique, the use of tests or qualities as variables) has its own demands to make in mental testing, and its own contributions to offer. These two techniques are the pillars of systematic psychometry, and each has its own foundations; each has its own objectives, involves its own premisses, and dictates its own requirements. It may well be pos-

sible to examine any given matrix of data *either* with respect to persons, *or* tests and qualities, as recent papers by Burt(7) seem to indicate; but in the two techniques under discussion, two different matrices are involved, and these are not interchangeable with respect to each other. Both matrices maintain the proper distinctions between a variable on one hand, and a statistical population on the other. And the data that go into the one technique can usually in no conceivable way enter into the other—for different objectives and premisses are involved. In the first paper the distinction between a variable and a statistical population led us to contemplate a new way of measuring ability, for which a new form of test had to be devised; ability was measured in a systematic way, independently of any statistical regard of persons as a population. In the present paper the same distinction has led us to conceive of a person's performance in a test as a statistical heap of items, which can be appraised for their mutual *significance* in his performance. This concept, born out of the present application of Q-technique, pertains to the immediate psychological situation; the analogous concepts that have long grown into maturity in r-technique, such as those of ability and capacity, refer instead to average, general, or controlled conditions.

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APPENDIX

The following is the list of qualities used in the above study; they are selected at random from a larger list. All refer to responses to the performance tasks; only short titles are listed. (The number *following* each quality is the score given to it in the appraisal of *one* student's work. See p. 239.)

1. Impulsive over-mover (10).
2. Timid use of material (−4).
3. Slow worker (−5).
4. Inhibited (0).
5. Hesitant use of material (−4).
6. Deliberate (2).
7. Predominant use of trial-and-error (3).
8. Little dependency on available clues (−3).
9. Works to a plan (1).
10. Systematic (−3).
11. Tries pieces at random in appropriate conditions (2).
12. Colour clues used first (2).
13. Builds shape with little use of colour clues (11).
14. Analytical (2).
15. Synthetical (10).
16. Capable and competent constructions (4).
17. Observant (4).
18. Good use of judgement (1).
19. Rapid worker (11).
20. Fluent (7).
21. A feeling for completeness (4).
22. Imaginative use of material (3).
23. Stiff, clumsy (−3).
24. Ponderous (−8).
25. Anxious (3).
26. Repeats errors (−4).
27. Compulsive (−4).
28. Perseverates (0).
29. Accepts inexact constructions for shape (−6).
30. Accepts inexact constructions for colour (8).
31. First regards available pieces for hints (−1).
32. Logical (−2).
33. Reasons (0).
34. Fits pieces without looking for a clue (3).
35. Accepts inexact constructions at conclusion (0).
36. Makes use of helpful abstractions (1).
37. Makes use of disturbing abstractions (4).
38. Practical (2).
39. Reserved in movement (−6).
40. Leaves well alone (−1).
41. Matter-of-fact (−3).
42. Dominant for shape (9).
43. Not abstractive for shape (1).
44. Competent space-sense (2).
45. Decisive (5).
46. Objective for size (0).
47. Steady output (6).
48. Fluent use of hypotheses (−2).
49. 'Fixed' ideas (1).
50. Speculative (2).
51. Wants identity (−2).
52. Judges aesthetically (−4).
53. Judges logically (−5).
54. Capable speed (6).
55. Over-critical (−3).
56. Inaccurate (−1).
57. Confused (−1).
58. Impulsive (5).
59. Talkative worker (−3).
60. Too-careful (−4).

(*Manuscript received* 18 *September* 1938)

ANSWER-PATTERN AND SCORE-SCATTER IN TESTS AND EXAMINATIONS[1]

By DAVID A. WALKER

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II. *An examination of the results of the Scottish Mental Survey* (pp. 249–253).
(1) *The verbal test* (pp. 249–251).
(2) *The pictorial test* (pp. 251–253).
III. *The relation of difficulty level and skewness of answer-pattern-differential* (pp. 254–255).
IV. *The variation of fit of answer-pattern-differential and score-scatter* (pp. 255–259).
(1) *Criticism of h* (pp. 256–257).
(2) *Attempt at a second method* (p. 257).
(3) *Indirect method using the idea of steepness* (pp. 257–259).
(4) *Conclusions* (p. 259).
V. *Summary* (pp. 259–260).
References (p. 260).

I. The predetermination of score-scatter by answer-pattern

The results obtained in an examination are often displayed in the form known as the score-scatter, which shows how the scores of the candidates are distributed over the possible range. From such a distribution the examiner can determine the average score, the standard deviation, and the skewness of scores, and from these figures may draw his conclusions on the nature of the population tested. But, as every examiner knows, the score-scatter is determined to some extent by the nature of the test applied; it is the product of the interaction of test and population, and as such it reflects the characteristics of both test and population.

The nature and extent of the dependence of score-scatter on the type of test used can be measured only when the phrase 'type of test' has been translated into terms as definite and measurable as those defining score-scatter. In previous papers in this *Journal* (1, 2), the author has suggested that the essential features of a test where the items carry unit score are best summarized in its answer-pattern, which is the table of the frequencies of correct answers to the items, when these have been placed in

[1] Read in part before Section J (Psychology) of the British Association at Nottingham on 7 September 1937.

ascending order of difficulty, the number of candidates being the first frequency given. It was shown in these papers that where the population tested is of the usual type, it is possible to predict from the answer-pattern the characteristics of the score-scatter with a fair degree of accuracy. The present paper contains a study along these lines of the results obtained in the Mental Survey of Scotland in 1932. This is followed by an account of further researches into the relation of answer-pattern and score-scatter.

II. AN EXAMINATION OF THE RESULTS OF THE SCOTTISH MENTAL SURVEY

In June 1932 the Scottish Council for Research in Education conducted a mental survey of an age group in Scotland, in which all the available children born in 1921 were tested by group intelligence tests. There has already been published a full report (3) giving details of the tests used and the results obtained, therefore reference is made in this paper only to those aspects directly concerning the present investigation, and one or two rather lengthy tables of figures of which use is made here are not reprinted from considerations of space.

In the form finally used for the survey the test consisted of two parts, a verbal part of seventy-one items, and a pictorial part of nine items. For our purpose these parts may be considered separately and referred to as the verbal and pictorial tests. It may be as well to point out here that the 1932 Mental Survey Test subsequently published (4) is not quite the same test, minor alterations having been made in the light of the results obtained. One of the alterations is rather interesting from the point of view of this paper; it is the re-ordering of the items in more accurate ascending order of difficulty, a procedure possibly suggested by the researches reported in these papers.

(1) *The verbal test*

This test consisted of seventy-one independent items, practically all of them carrying unit score. The score-scatter was obtained from the papers of all the candidates, a total of 44,210 boys and 43,288 girls. The answer-pattern, on the other hand, was derived from the papers of 500 boys and 500 girls, the cases being selected at random by the marking committees. The random nature of the selection was tested and found satisfactory. This conclusion is strengthened by a test made by the present author; the answer-patterns of boys and girls were found to differ only to the extent expected through random sampling. The test used for this purpose was Pearson's test of Goodness of Fit, as revised by Fisher (5).

The answer-pattern obtained by combining these sets of data is shown in Fig. 1. The figures from which it is constructed are to be found in the report ((3), pp. 87–8). This type of answer-pattern might be called steep, in that the items progress smoothly from extreme ease to extreme difficulty, covering the whole range. The answer-pattern-differential, which is the table of differences between adjacent items of the answer-pattern, is therefore roughly a rectangular distribution, i.e. each value of

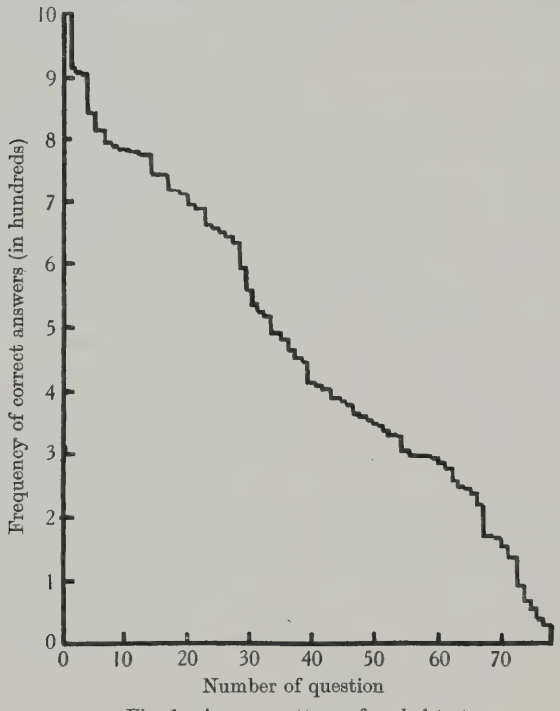

Fig. 1. Answer-pattern of verbal test

the abscissa has the same frequency. When the values of the answer-pattern-differential have been grouped in tens, as was the case with the scores, the coefficient of skewness as calculated by the third moment formula is found to have a small positive value 0·20. (The application of moment formulae to these distributions has been discussed in previous papers.) On the basis of the theory put forward in these papers the score-scatter should have similar characteristics; in particular its skewness should be slight.

The score-scatter obtained from the total population of 87,498 candidates is shown in Fig. 2, the scores being grouped in intervals of ten. The

distribution is of approximately normal type, there being a slight negative skewness −0·17. This comparative lack of skewness can be attributed to the lack of skewness in the answer-pattern-differential. On the other hand, no feature of the answer-pattern-differential can account for the normal or Gaussian nature of the score-scatter. This normality is a property of the population tested and not of the test used.

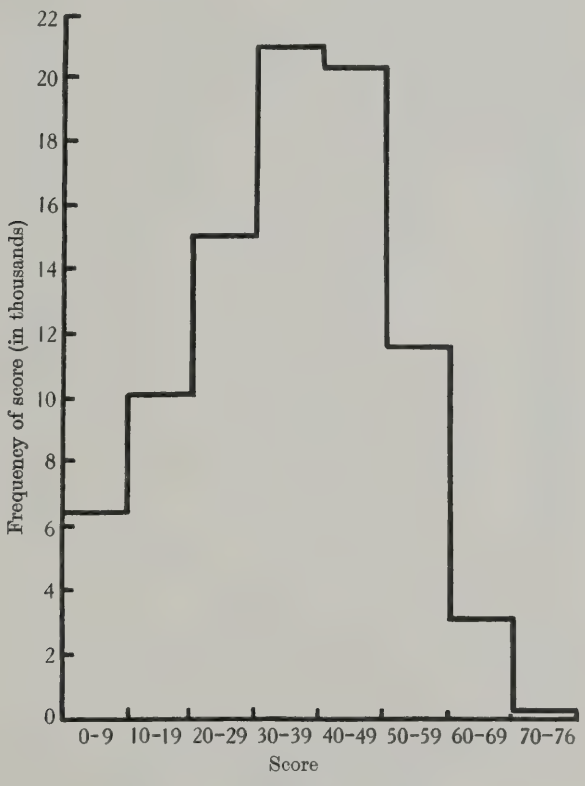

Fig. 2. Score-scatter of verbal test.

(2) *The pictorial test*

This test consisted of nine items, each carrying unit score. It was the sole survivor of three tests of the pictorial type, the other two having to be eliminated for various reasons. The answer-pattern was obtained from the papers of 450 boys and 450 girls selected at random as before. Here again the random nature of the selection was found to be satisfactory, and the application of Pearson's test of Goodness of Fit showed that the differences between the two patterns could be attributed to the effects of

sampling. The combined answer-pattern is shown in Fig. 3; the figures from which it is obtained are included in Table I.

It will be observed that the test was a very easy one; it was, in fact, intended to give the weaker candidates scope. The answer-pattern is flatter than that of the verbal test, and produces an answer-pattern-differential

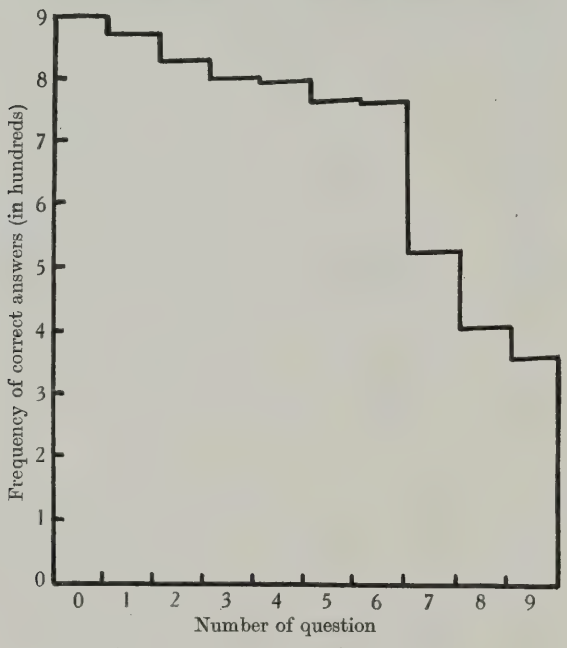

Fig. 3. Answer-pattern of pictorial test.

Table I

Item	0	1	2	3	4	5	6	7	8	9
Frequency of correct answers	900	868	829	791	761	800	764	363	519	403
Answer-pattern	900	868	829	800	791	764	761	519	403	363
Answer-pattern-differential	32	39	29	9	27	3	242	116	40	363
Score-scatter (in hundreds)	16	16	21	27	30	52	135	186	205	187

with a tail to the left and a hump to the right, as shown in Table I. Such a distribution has a strong negative skewness which in this case is $-1{\cdot}20$.

The score-scatter obtained from the papers of all 87,498 candidates has a coefficient of skewness $-1{\cdot}33$. Its histogram is shown in Fig. 4 and the frequencies to the nearest hundred are given in Table I. Now this strongly skewed score-scatter was obtained from the group of

candidates which yielded the normal score-scatter of Fig. 2. The sole difference has been in the test used, and, as predicted, the strong negative skewness of the answer-pattern-differential has produced an equally great negative skewness in the score-scatter.

In Table I the figures of the answer-pattern-differential are not strictly comparable with those of the score-scatter, since their totals are

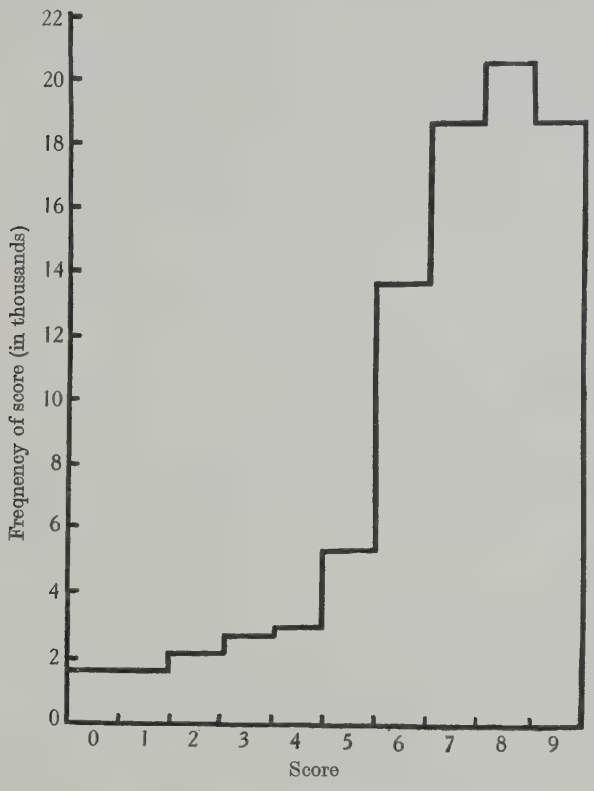

Fig. 4. Score-scatter of pictorial test.

900 and 875 respectively, but the obvious resemblance of the two sets is little affected by this slight difference.

The results of these tests show the reality of the connexion between answer-pattern and score-scatter. The connexion is not only real but useful. Let us suppose that in the try-out of a test such as the one considered there was obtained a score-scatter of a type not desired by the examiner. Then the task of adjusting the test-paper is changed by the application of this theory from a trial and error basis to the methodical adjustment of the answer-pattern to match the score-scatter desired.

III. The relation of difficulty level and skewness of answer-pattern-differential

The strength of the relation between the answer-pattern-differential and the score-scatter of a test may be measured in one aspect by correlating skewness of answer-pattern-differential with skewness of score-scatter over a number of tests. Such an experiment was reported in a previous paper (2) and the results showed the correlation to be of the order 0·6–0·8. It was pointed out in the same paper that a third variable, the difficulty level of the test, was also highly correlated with skewness of score-scatter, and, to make matters still more complicated, was highly correlated with skewness of answer-pattern-differential. For example, in one series of tests the correlation of skewness of score-scatter (S) with skewness of answer-pattern-differential (S') was 0·617; that of S with difficulty level d was 0·514; while the correlation between S' and d was 0·846. The regression equation predicting S from S' and d was

$$S = 0{\cdot}64S' - 0{\cdot}03d,$$

indicating that S' is the all-important factor in the prediction of S.

Unfortunately for the certainty of this conclusion, the difference between $r_{SS'}$ and r_{Sd} is not statistically significant. It would seem at first sight that another sample of tests might yield a value of r_{Sd} greater than $r_{SS'}$, and then the regression equation would give d as the all-important factor in predicting S. On the other hand, all but one of the sets of tests used gave values of $r_{SS'}$ greater than r_{Sd} though not significantly so. This was the uncertain position when the last paper was published.

Further work has yielded a solution to this problem, the key to which lies in the high correlation of S' and d. In measuring S' and d we are to some extent measuring the same thing. It will now be shown that the difficulty level d is an approximation to S', the skewness of the answer-pattern-differential.

S' is calculated from the answer-pattern-differential by the method of moments illustrated in the previous paper (2). If the test is flat, i.e. if all the items are equally difficult, the level being d, then calculation of S' produces, after simplification, the result $\dfrac{2d-1}{\sqrt{\{d(1-d)\}}}$, a function of d which we may denote by $\phi(d)$. The skewness of the answer-pattern-differential of a flat test can thus be calculated directly from its difficulty level. Now it can be shown mathematically that for *any* test, a first approximation to S' is given by $\phi(d)$. The goodness of the approximation varies with the type of the answer-pattern. The process of approximation may be carried

a stage further. Since d does not usually differ greatly from 0·5 a first approximation to $\phi(d)$ is given by $4d-2$. The error introduced by the last approximation is easily calculated; for values of d between 0·3 and 0·7 it is less than 10 %. For correlation purposes d and the linear function $4d-2$ are interchangeable; thus d is an approximation to $\phi(d)$, which in turn is an approximation to S'.

Thus the well-known rule-of-thumb method of skewing score-scatters by the variation of difficulty level appears as an approximation to the basic principle, that the skewness of a score-scatter is partly controlled by the skewness of its answer-pattern-differential. Since it is an approximation this use of difficulty level cannot be expected to yield such accurate results as that using the answer-pattern-differential; this is demonstrated by the diminution in the correlations found in two series of tests, shown in Table II.

Table II

	First series	Second series
r_{SS}	0·628	0·836
$r_{S\phi}$	0·576	0·816
r_{Sd}	0·561	0·790

For this reason a better prediction of S will always be obtained from S' than from d. From a study of the whole answer-pattern one gains a better idea of the probable skewness of the score-scatter than would be obtained from a knowledge of the test's difficulty level alone.

An interesting commentary on the above chain of reasoning is provided by the regression equation connecting S' and d, calculated from the results of one series of tests. When S' and d are measured in their original units, this equation reads $S'=4{\cdot}10d-2{\cdot}07$, which compares well with the approximation $4d-2$ for S'. The absence of values of d outside the range 0·19 to 0·66 probably accounts also for the apparent anomaly that S was found to be correlated in linear fashion with such varying functions of d as d^3, $\phi(d)$ and d itself. For these tests of linearity the method employed was that given by Fisher(5), replacing Blakeman's criterion, which was used in the previous paper.

IV. The variation of fit of answer-pattern-differential and score-scatter

It was shown in the first paper of this series(1) that the answer-pattern-differential and score-scatter of a test are identical in the particular case referred to as 'unig', where each candidate's score is compiled of answers to the easiest questions. The experiments previously reported

and the results of the Scottish Tests analysed in this paper, show that, in the general case where random answering is present, there is still some measure of relation or correspondence between the two distributions.

One measure of correspondence that has been used is the correlation coefficient of corresponding statistics (e.g. the coefficient of skewness) of the distributions. The calculation of these correlations implies the existence of several tests applied to the same or similar populations, and the results obtained apply to the group of tests as a whole. To study the variation of fit of answer-pattern-differential and score-scatter from test to test, it is necessary to devise some measure of this fit applicable to the results of a single test.

First it may be pointed out that the answer-pattern-differential and score-scatter are usually so removed from identity that no use can be made of Pearson's Test of Goodness of Fit. In any case this test can be used only to decide whether or not two distributions may be regarded as identical save for the effects of random sampling; it cannot be used to measure the degree of resemblance as the title "goodness of fit" might suggest.

(1) *Criticism of h*

In the first paper of this series (1) an account was given of a coefficient devised for this purpose. Denoted by h, it was termed the coefficient of 'hig', being intended to measure the poorness of fit that accompanies higgledypiggledy answering. Further research has shown that the definition of h is based on assumptions that involve over-simplification of the problem. The word hig was used to cover three conceptions which at that time had not been observed to be distinct. These are:

(i) randomness of answering, for which the term hig should properly be reserved;

(ii) poorness or goodness of fit of answer-pattern-differential and score-scatter, for a measure of which we are seeking;

(iii) control of score-scatter by answer-pattern-differential, i.e. the fit of the score-scatter to a score-scatter calculated from the answer-pattern-differential but not necessarily identical with it.

These concepts are linked by the fact that complete absence of random answering implies perfect fit and perfect control. But we must abandon the assumption, made in the definition of h, that the poor fit that accompanies complete randomness of answering, as shown by a flat answer-pattern, is due to its random nature, and that there is in this case no control of the score-scatter by the answer-pattern. Examination of the answer-patterns and score-scatters of many tests has made it clear that the poor fit accom-

panying completely random answering is due mainly to a property of score-scatters ignored in defining h, the tendency of all score-scatters to the normal or Gaussian type of distribution. Thus the good fit obtained with steep tests B and C of the first paper (1), and the poor fit found with test A there, may be accounted for by the answer-pattern-differentials of B and C being already akin to normal distributions, while that of A was far from normal. Even where poor fit is shown, as in these flatter tests, there is evident some control of score-scatter by answer-pattern-differential; occasionally even the double hump characteristic of the answer-pattern-differential of a flat test is reproduced in the score-scatter.

(2) *Attempt at a second method*

This line of argument led to the devising of a second coefficient to measure the relationship between answer-pattern-differential and score-scatter. The score-scatter obtained was regarded as the resultant of the answer-pattern-differential and a normal distribution characteristic of the population tested, and the relative strengths of these two factors were measured. Although results were obtained for one set of tests, the method is not generally useful as the theoretical basis for the characteristic normal distribution is insufficient for most tests. Other coefficients were devised, tested, and rejected for similar reasons.

(3) *Indirect method using the idea of steepness*

It would seem then that failure must be recorded in our attempts to measure that correspondence which can often be observed in the results of a test. There is, however, one other method of attack; though it does not yield a solution of the problem the results seem to dispose of it in another way by suggesting that in general it does not arise; that in the tests in common use the fit or control we seek to measure varies only slightly. For this avenue of attack we return to the idea of steepness.

It was proved in an earlier paper (1) that the probability of unig (which implies perfect fit of answer-pattern-differential and score-scatter) was greater with steep tests than with flat tests. If we have failed to differentiate tests directly by the degree of correspondence between their answer-pattern-differentials and score-scatters, we may attempt to do so indirectly by measuring their steepness.

The definition of steepness used in the proof mentioned above is merely the statement that steepness implies large differences in the difficulties of the items, as measured by the numbers of correct answers. This implies (i) that the number of items is small; (ii) that these items make

the greatest use of the available difficulty range. The second implication will be considered first. One way to make use of the whole difficulty range is to make the items progress uniformly in difficulty from extreme ease to extreme difficulty, so that the graph of the answer-pattern is a straight line. The curve which makes the probability of unig a maximum is not strictly a straight line, but the whole calculation is of such mathematical difficulty, and the result so far as obtained so nearly a straight line, that this has been adopted. Now a straight line is the shortest distance between two points. Any other answer-pattern including the initial value n_0, the number of candidates, and falling to zero after the last item, has therefore a longer path, the length of which is a measure of the departure from steepness. On this reasoning there was based a coefficient of steepness which could be used to compare the steepnesses of tests which had the same number of items. Each of the forty-one tests previously referred to had ten items, and by the use of this measure of steepness they were divided into two groups of steeper and flatter tests, the dividing line being fixed to place approximately equal numbers of tests in each group. Each group then provided data from which could be calculated the correlation of corresponding statistics by the method used in the previous paper (2).

When this was done the results were as follows: The correlation of the standard deviations of answer-pattern-differential and score-scatter for the twenty steeper tests was 0·78; for the twenty-one flatter tests it was 0·83. The difference is not statistically significant. In the matter of standard deviation the flatter tests show a fit which is better, but not significantly so. The correlation of the skewness of the answer-pattern-differential with the skewness of the score-scatter for the twenty steeper tests was 0·80; for the twenty-one flatter tests it was 0·59. Again the difference is not statistically significant. Here the better fit is obtained from the steeper tests. From the scanty data at our disposal it would seem then that the control of score-scatter by answer-pattern-differential is no greater in the case of the steeper group than in the case of the flatter group.

This conclusion, which at best is only tentative, is supported by the result obtained in a second experiment with the results of the forty-one tests. The steepness coefficient was correlated with the coefficient already mentioned measuring the relative influence of the answer-pattern-differential and a hypothetical normal distribution. The correlation was found to be 0·218, which though positive is not significantly different from zero, being derived from forty-one pairs of values. The steeper tests do not

show a significantly greater degree of control by the answer-pattern-differential.

(4) *Conclusions*

It would appear then that for tests of ten items or more, the control of score-scatter by answer-pattern-differential is practically independent of the steepness of the test. This is rather a welcome conclusion since it simplifies matters somewhat. To produce certain types of score-scatter it is necessary to use answer-patterns of the flatter type. If the certainty of control decreased as the pattern became flatter, the position would be much more confused than it is. As far as these results go, they show that the flatter answer-pattern exerts about as much control over the score-scatter as does any other type.

These conclusions are for tests having ten items or more. If the test has very few items the position is altered. In such a test the differences in difficulty between the items can be made large, so that the test is really steep in the sense that the probability of unig is high. In any ten-item test the probability of unig must always be so low as to be negligible, however well the items use the range of difficulty. To produce probabilities of the order of 0·2 the test must have no more than four items, while probabilities of the order of 0·6 restrict the number of items to three. But this is a type of test few examiners would care to use; it would very easily give rise to difficulties through misunderstandings, prior knowledge, and guesswork, all of which tend to be smoothed out in a test with many items.

We are thus in the position of having gained upon the roundabouts what we lost upon the swings. We have lost hope of constructing useful unig tests, but have gained a fair degree of certainty that the flatter tests show as great a correspondence between answer-pattern and score-scatter as do the steeper tests.

V. SUMMARY

1. The score-scatters and answer-patterns obtained from the tests used in the Scottish Mental Survey show on examination that relation which has previously been predicted and illustrated by smaller scale experiments. A particular point of interest is the production by the same population of score-scatters differing widely in skewness under the influence of appropriate answer-patterns.

2. The rule that a difficult test tends to produce a positively skewed score-scatter is shown to be a particular case of the general principle that

skewness of score-scatter is highly correlated with skewness of answer-pattern-differential.

3. As far as can be ascertained from the available data, the tie between answer-pattern and score-scatter is equally strong for steep tests and for tests that tend to the flat type.

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(*Manuscript received* 12 *January*, 1939)

PUBLICATIONS RECENTLY RECEIVED

Theories of Sensation. By A. F. Rawdon-Smith. Cambridge University Press. 1938. Pp. xiii+137. 10*s.* 6*d.* net. New York: Macmillan. $2.75.

It is not easy for psychologists to keep abreast of the rapid advances which are being made in the physiology of the special senses and central nervous system. Dr Rawdon-Smith's book will therefore be of very great value for its surveys of the most important work, and especially the recent investigations, on vision and hearing. The author possesses a remarkable talent for summarizing intricate theories and experiments both concisely and clearly. His treatment will be too advanced for the majority of students, but will be extremely useful to those who are already familiar with the ordinary information about the special senses contained in standard psychological textbooks.

If one has a complaint to make, it is on the ground that the book is essentially a physiological, not a psychological, monograph. As the title indicates, it discusses visual and auditory sensations, without any regard for the psychologist's doubts and difficulties as to the reality of sensations. For example, there is no mention of Katz's work in the detailed account of colour vision, nor any indication that the 'bundle' and 'constancy' hypotheses of nerve conduction and cortical activity are suspect. The physiologist is apparently as atomistic in his approach as ever he was. This criticism does not, of course, reduce the value of the brilliant experimental work of recent years which Dr Rawdon-Smith describes (and in which he has himself played no small part). But it does mean that such work tells us scarcely anything fresh about the psychology of visual or auditory perception. And it is unfortunate that the gap between physiology and psychology should, apparently, be widening rather than decreasing.

The book is excellently produced and printed. The cost would seem rather excessive, were it not that the book contains so large a quantity of highly concentrated information. It is a pity that the bibliography is only a skeleton one. Both the titles and the full page references of books and articles would often be useful to the reader.

P. E. V.

Frustration and Aggression. By John Dollard, Leonard W. Doob, Neal E. Miller, O. H. Mowrer and Robert R. Sears. New Haven: Yale University Press (London: Humphrey Milford). 1939. Pp. ix +209. 9*s.* net.

It is somewhat difficult to understand the aim of this book. The authors begin with the postulate that "aggression is always a consequence of frustration" and, moreover, that frustration always leads to some form of aggression. There are four supplementary principles: the amount of aggression varies with the amount of frustration; the strength of inhibition of an act of aggression varies positively with the amount of punishment to be anticipated as a consequence of that act; inhibited aggression becomes displaced or altered, either as regards its object or as regards its form; the occurrence of an act of aggression will reduce the 'instigation' to subsequent aggression. All these tenets are illustrated by numerous examples drawn from everyday life, particularly the development of children, from criminology, sociology and ethnology. So far so good; there are few people who would deny that acts of aggression frequently result from frustration and occur in accordance with the principles described above. Moreover, it is

most interesting and valuable to have the various instances arranged and systematized within the same scheme. But where do we go from here? Since we learn nothing about the nature of frustration and aggression, their psychological as apart from their environmental causation, we cannot tell why certain of the above principles operate in some instances, others in others. Nor could we account for the numerous instances in which neither the above principles, nor the original basic hypotheses, appear to operate. The truth is that, in the absence of some causal hypothesis, however tentative, the average reader is at a loss as to how to schematize his knowledge. And the psychologist must go on adding to his pile of facts about behaviour, not knowing how to distinguish between the significant and the trivial. The authors acknowledge their debt to Sigmund Freud; would that it had been greater!

Technique of Analytical Psychotherapy. By Wilhelm Stekel. Translated from *Technik der analytischen Psychotherapie*, by Eden and Cedar Paul. London: John Lane, The Bodley Head. 1939. Pp. xvii + 408. 21*s.* net.

Although this book is intended primarily for those who practise, or hope to practise, psychotherapy, it gives a plain and comprehensive description of the practice of psycho-analysis which will be of interest and value to any psychologist. The latter will be particularly glad to be relieved of the mythological hocus-pocus which emanates from so much of the writing of the Freudian psycho-analysts. Not that Dr Stekel in any way underrates the paramount importance of Freud's contribution to psychological knowledge. But he does speak out strongly against some of the Freudian shibboleths: the stressing of infantile sexual traumata and of the castration complex in neurotic conflict; the necessity of apparent passivity on the part of the analyst; and the advisability of very lengthy and exhaustive analyses. Though convinced of the importance of intuitive interpretation and of active intervention in the course of the analysis (and thereby stressing the importance of the analyst's own character and of his awareness of his own 'blind spots'), Dr Stekel roundly condemns the atmosphere of esoteric mystery and the slavish acceptance of authority and dogma which have crept up among many Freudian analysts. But is it possible that the attribution of every form of neurotic conflict to a sexual origin is a 'blind spot' of Dr Stekel's?

The author's desire not to be dogmatic, to qualify and illustrate all his conclusions has, however, rendered his presentation somewhat prolix and discursive; and the reader may at times find it hard to follow the thread of his argument. The book would be improved by pruning. There is a really good chapter on the Psychoanalysis of Obsessional Disease, previously read as a separate paper, and presented more logically and systematically than the remainder. But the reader who perseveres will be rewarded by a real understanding of the practice of psychoanalysis, its dangers, its difficulties and its manifest value.

The Language of the Dream. By Emil A. Gutheil. New York: The Macmillan Company. 1939. Pp. 286.

The author of this book is an associate and disciple of Stekel, and he gives a full, clear and able account of dream analysis by the 'intuitive', 'active' and 'economical' technique employed by Stekel. In this, little, if any, use is made of the patient's free associations to the dream content, although analysis is of course made in the light of the particular circumstances of the case. The book gives detailed descriptions of dream mechanisms and symbolisms and of the analytical interpretations of many dreams of normal, neurotic and psychotic cases. Finally, a short but illuminating chapter contrasts some interpretations made by Stekel and his followers, with the corresponding

interpretations made by Freud, Adler and Jung. The reader is left with the uneasy feeling that the interpretation is mainly a function of the analyst's preconceived opinions and theories. Stekel's interpretations are, in general, the least fantastic, and his method has the great advantage of making psychotherapeutic treatment more rapid. There is no doubt that in his hands dream analysis is an efficient tool. But this does not substantiate the ultimate validity of his readings of the language of the dream.

New Ways in Psychoanalysis. By KAREN HORNEY. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1939. Pp. 313. 12*s.* 6*d.* net.

In the light of her experience as a psychoanalyst and of the doubts which it set up as to the full validity of Freudian psychoanalytic theory, Dr Karen Horney sets out to make a critical revaluation of that theory, and to eliminate its more erroneous features. She condemns particularly Freud's adherence to instinctivist and genetic explanations and derivations, and to his insistence on a purely biological determination, e.g. by anatomical sex differences as in the castration complex. Dr Horney, on the contrary, considers that "the relevant factor in the genesis of neuroses is neither the Oedipus complex nor any kind of infantile pleasure strivings, but all those adverse influences which make the child feel helpless and defenceless....Narcissistic, masochistic, perfectionistic trends...are not derivatives of instinctual forces, but represent primarily an individual's attempt to find paths through a wilderness full of unknown dangers. The manifest anxiety in neuroses is not the expression of the 'ego's' fear of being overwhelmed by the onslaught of instinctual drives or of being punished by a hypothetical 'super-ego', but is the result of the specific safety devices' failure to operate."

This thesis Dr Horney develops most ably and persuasively. But her presentation is somewhat marred by her failure to cite any detailed case histories; she appears to select from them such isolated features as will best prove her case. This omission may be necessary in part in order to save space. But the reader, interested and impressed though he undoubtedly will be by the inherent psychological probability of her argument, would like to study in more detail the evidence upon which it is based.

Play Therapy in Childhood. By C. H. ROGERSON. Oxford Medical Publications. Oxford University Press (London: Humphrey Milford). 1939. Pp. 64. 3*s.* 6*d.* net.

This brief monograph describes work carried out mainly in the Department of Psychological Medicine at Guy's Hospital on the effects of play therapy on certain rather difficult cases of neurotic disturbance in children. The effect of the treatment was apparently beneficial; but it is not possible to say how far this was due to the actual 'play' treatment, and how far to the friendly relations established between the child and the therapist. The reports of cases and the discussion are too slight to add much to our knowledge of the function of play in childhood.

The Intelligence of a Representative Group of Scottish Children. By A. M. MACMEEKEN. London: University of London Press, Ltd. 1939. Pp. xvi+144. 5*s.* net.

When the survey of the intelligence of a whole age-group, described in *The Intelligence of Scottish Children*, was carried out, at the same time a sample of 1000 children from the group was given the 1916 Stanford Revision of the Binet tests. But this smaller group did not form a random sample. Therefore, it was decided by the

Scottish Council for Research in Education to test a further group of about the same size, but selected from all areas and all social classes in Scotland, with the Stanford Revision and also with a Performance test scale. In addition, all the Binet tests and most of the Performance tests were performed by a single investigator, Miss MacMeeken, to avoid variations in assessment between different investigators. In this way a set of very valuable data was obtained; and this book gives a lucid account of some of the results of this most interesting and important experiment. The distributions of (Binet) intelligence and of Performance test scores are discussed, and are compared as between boys and girls, and as between urban and rural areas. Further results will appear in due course.

Biographies of Child Development. By Arnold Gesell, Catherine S. Amatruda, Burton M. Castner and Helen Thompson. London: Hamish Hamilton, Ltd. 1939. Pp. xvi+328. 15*s*. net.

This book contains in its first part follow-up studies of typical cases described in Dr Gesell's previous publications; and in its second part, individual case studies illustrating certain peculiarities in growth and development. But all these studies are intended to show that, by using Dr Gesell's technique of assessing the degree of development of certain main characteristics, it is possible in infancy and early childhood to predict with considerable accuracy what later development will occur in the adolescent and adult. Since physical, intellectual and motor development are comparatively easy to measure, it is these which can be most accurately predicted. Social development can also be described, but the development of emotional characteristics and of the personality is shown only vaguely. In consequence, the reader is left with the impression of separate lines of development of isolated characteristics which never become synthesized into a completed personality. Useful as Dr Gesell's technique undoubtedly is in predicting the future development of the more obviously apparent characteristics, it can as yet tell us little beyond the use of general terms such as 'stable', 'immature', 'excitable', etc., about the kind of 'person' the child will become.

The type face employed is too 'arty' and unsuitable for printing this kind of book, and, moreover, interferes distinctly with its legibility, at least for some readers.

Child Psychology. By Fowler D. Brooks. London: Methuen and Co., Ltd. 1939. Pp. xxx+600. 12*s*. 6*d*. net.

This book provides an admirable compendium of the vast and growing American literature on Child Psychology. It only touches on the work of the Austrian school, and omits all reference to that of the Gestalt psychologists and of English child psychologists. Since the American literature is mainly behaviouristic in outlook, the child's overt behaviour and physical characteristics are dealt with more adequately than are its less obvious psychological characteristics. Motivation, emotion and personality are discussed rather superficially, with insufficient reference to underlying basic structures and mechanisms.

Within its limitations, the book is a good one of its kind, and should be useful to the specialist in child psychology as a source of reference. But it would be a mistake to let it add more weariness to the flesh of the English psychological student. And it is not up to the standard of Messrs Methuen's "Contributions to Modern Education" series. The introductory 'puff' by an un-named editor is unnecessary.

Nursery School Education and the Reorganization of the Infant School. By Olive A. Wheeler and Irene G. Earl. London: University of London Press, Ltd. 1939. Pp. 172. 5*s.* net.

At a time when schools are being disorganized and educational reform retarded, and when parents find it increasingly difficult to spend much time with their children, it is most fitting that the case for the Nursery School should be pressed; and that every attempt should be made to substitute it for the traditional Infant School. The Nursery School not only relieves the harassed mother during the daytime of the care of little children aged 3 to 6, but also gives them opportunities of free and healthy living which they would otherwise miss, and helps them to acquire good habits of eating, sleeping, cleanliness and social co-operation.

In this inexpensive and charmingly illustrated book, Prof. Olive Wheeler describes the psychological characteristics of the child and shows how they are developed and advantaged by the Nursery School. Miss Earl describes in more detail the school activities. Though there is nothing novel in the book, it is clear and readable, and should provide excellent propaganda for the spread of Nursery Schools.

Religion and the Growing Mind. By Basil A. Yeaxlee. London: Nisbet and Co., Ltd. 1939. Pp. x+224. 7*s.* 6*d.* net.

The first chapter of this work is largely polemical. Its attempt is to show that those modern psychological theories which seem to imply that religious impulses and experience have a perfectly naturalistic explanation are unacceptable. As would be expected, the main arguments are directed against Freudian and allied interpretations. In subsequent chapters general mental development from early years through adolescence to maturity is followed and for each stage of development the author tries to indicate clearly the characteristic appeal and forms of religion. The final parts of the book endeavour to apply all the conclusions reached in a practical way to problems of the home, the school and the world. The author has read widely and makes great use, sometimes in a rather scrappy fashion, of a large range of authorities. The book is sincere, honest and fair; but it is not very original. It has no difficulty in showing how extremely deep-rooted in man are religious yearnings and beliefs. But the critical reader will realize that when it passes to statements of a specific kind of reality answering to those yearnings and beliefs, he is as much beyond psychology as are his opponents who assert that there is no such reality. In fairness to the author it should be said that he is himself more frankly aware of this than many of those who have asserted the illusory character of religion, basing their assertion on psychological grounds, have often appeared to be.

Fit after Forty. By Clement Jeffrey. London: Frederick Muller, Ltd. 1939. Pp. 213. 5*s.* net.

This is a book of good advice. It need not have the effect of making people worried, for it is sensible enough, and in no way extreme or frightening. The topics discussed are: Diet, Constipation, Getting through the Winter, the Change of Life and Mind after Forty. The final chapter is the only specifically psychological one, and at any rate it is some recommendation to say that it does not contain anything that will greatly surprise the psychologist. It is rather platitudinous, but there are always plenty of people to whom platitudes come for the first time and who may benefit greatly from them.

Nouveau Traité de Psychologie. Par GEORGES DUMAS. Tome VI. Paris: Félix Alcan. 1939. Pp. iv+544. 125 fr.

The sixth volume of this very fine 'new treatise' deals with the systematic functions of the affective and active behaviour of human beings. Prof. Dumas has been both skilful and fortunate in his choice of collaborators, and the result is a volume which more than maintains the high standard of its predecessors. The topics and authors represented in this book are: *La Logique des Sentiments* and *Les Passions*, by L. Dugas; *l'Évolution, la Spiritualisation et la Socialisation des Tendances*, by F. Challaye; *l'Amour et la Haine*, by D. Lagache; *les Sentiments Sociaux et les Sentiments Moraux*, by C. Davy; *les Sentiments Religieux*, by C. Belot; *les Sentiments Esthétiques et l'Art* and *l'Invention et le Génie*, by H. Delacroix; *les Volitions*, by Ch. Blondel; and *Psychophysiologie de l'Effort Physique*, by H. Laugier and W. Liberson. It will be seen that all of the essays, with the exception of that dealing with the psycho-physiology of muscular work—and this, by the way is an extremly good and well-documented survey of experimental work—call rather for delicate powers of psychological analysis and a wide knowledge of human life than for a strict laboratory procedure. And anybody who is acquainted with the brilliant stream of French contributions to the study of feeling and emotion from Ribot onwards, will approach this book with high hopes. Nor is he likely to be disappointed. For the essays are, for the most part, delightful to read, full of literary as well as of scientific allusions, and themselves artistic as well as penetrating. It is much to be hoped that this volume will be widely read and fully appreciated in this country as well as in France.

La Psychologie Expérimentale en Italie, École de Milan. Par A. MANOIL. Paris: Félix Alcan. 1938. Pp. viii+489+16 plates. 80 fr.

As most English readers well know, Prof. A. Gemelli, at the University of Milan, has for a good many years been conducting important researches in experimental psychology. This book gives a very excellent account of the main groups of these investigations, with all the necessary references to the original publications of Gemelli and his associates. The studies dealt with are: emotional phenomena, researches into psychophysical methods and problems, comments on the 'thinking' horses of Elberfeld, contributions to the study of perception, the electro-acoustical study of language, the selection of air-pilots, and general industrial applications. At the conclusion of the volume the general point of view of the Milan school is briefly and clearly indicated. For those who cannot read or obtain the original Italian publications this book will be of the greatest value, for it is reliable and authoritative. There is a very good bibliography.

L'Automatisme dans l'Art: Idée et Mouvement. Par ANTOINE GIRAUD. Paris: Librairie Marcel Rivière. 1939. Pp. 127. 12 fr.

Dr Giraud may be right or wrong, but the essay which he has produced is full of interest in either case. The distinction which we commonly make between certain processes which are consciously controlled and others which spring from the unconscious is, he considers, a misleading one. There is but one source of all products of the mental life, and this is the totality of ideas which come, how it is hard and perhaps impossible to say, to the producer. Moreover, however he may appear to work, what he produces is the expression of the laws of interrelation of these ideas among themselves and in relation to him. Ideas having accumulated reach a certain state of tension which may be lesser or greater according to the amount of control that is being exercised, but when they break through the control then comes the moment or the time of expression.

Since intoxications of various kinds may weaken or release control it often happens that intoxication is the immediate occasion for the production of imaginative work. All this is clearly set forth and illustrated by many quotations from people of genius in all fields of art. A final and rather long chapter, endeavouring to show that ideas and movement are at bottom identical, so that, for example, if the former are let alone to have free play, a form of active expression must result, is not as convincing or as satisfactory as the rest of the essay. But the book is worth reading, and its most apposite quotations will be of great value to anybody who is on the look-out for any material with which to illustrate any study of imaginative inspiration.

L'Épithymologie (*La Désirologie*). Par JEAN URBAN. Paris: Félix Alcan. 1939. Pp. 359. 50 fr.

Epithymology is, according to the author, that branch of social psychological science which endeavours to lay bare the static and dynamic laws regulating the interrelations of desires, the connexion of desires with the varying internal and external environments and the phenomena arising therefrom. Also, from a practical point of view epithymology endeavours so to apply the laws as to achieve a free and fully voluntary existence. The volume deals first with feeling and desire, secondly with the laws of desires, and thirdly with will and the laws of conscious activity. As the title indicates, the treatment is systematic, logical, analytic, rather than experimental or even empirical. It is a book for the psychologist to read critically. If he treats it, not as a definitive treatise, but as an introduction to a very large number of little investigated problems, he will be able to gather a great many ideas and suggestions which could be submitted to more factual and immediate test.

La Mémoire et la Vie: Essai de Défense du Mécanisme Psychologique. Par E. AUGIER. Paris: Félix Alcan. 1939. Pp. 251. 30 fr.

It is perhaps a little curious that while this book is a sustained defence of strictly empirical methods in psychology, and an attack upon all contemporary tendencies to take refuge in rather vague and qualitative principles of explanation, it has itself very little to do with directly recorded data and moves almost wholly in the realm of concepts. Dr Augier argues that precisely the same general framework of ideas that are used in physics, chemistry and the other natural sciences apply in just the same ways to all psychological problems. This he considers to be most evident in regard to the study of memory which he pursues, in a very general manner, first as he finds it illustrated in inorganic matter, then in general biology, then in behaviour and finally in human psychology. Memory he considers always to have two forms: "inscription des traces du passé, évocation de ces traces sous forme de souvenir," and the two together "forme une base explicative complète et commode pour tous les faits psychologiques". It is a pity that this view, which is certainly less uncommon than the author seems to believe, is argued in a somewhat controversial manner rather than demonstrated. Nevertheless, the essay contains much to interest any psychologically trained reader, and perhaps later Dr Augier may put his general arguments and proposals into a more concrete and experimentally illustrated form.

Principes d'une Psychologie des Tendances. Par A. BURLOUD. Paris: Félix Alcan. 1938. Pp. 430. 60 fr.

Here is a most interesting book which deserves to attract the attention of many English readers. M. Burloud has set before himself the task of developing a complete and fundamental point of view for the study of psychology. His main affiliations are with Maine de Biran and with the development of a standpoint concerned less with the content of mental processes than with a sustained attempt to work out the characteristics, interrelations, forms of operation and conditions of these processes. At all

possible points he is willing to call in the aid of experimental study, and many of the interesting experiments reported are original. Perhaps the most interesting parts of his study are those concerned with schemata, their development and their functions. He covers a very wide field, and deals not only with all the main forms of general mental activity, but with the specific evolution of active directed processes in sensation, in behaviour, in perception, in recall and in thinking. Whether or no his views about the final character of psychological explanation are accepted, his discussions and his illustrative material will be found stimulating by any well-informed psychological reader.

Conscience et Mouvement: Étude sur la Philosophie Française de Condillac à Bergson. Par GABRIEL MADINIER. Paris: Félix Alcan. 1938. Pp. 481. 65 fr.

Anybody who is interested in the use made by the outstanding French philosophical and psychological writers of the notions of activity and of dynamic ideas will find an excellent critical account in this book. The general development of these ideas is traced from Condillac to Bergson, and, from the psychological point of view, some especially interesting remarks are made about Fouillée and Renouvier. Naturally the contributions of Maine de Biran receive a great amount of attention.

Conscience et Amour: Essai sur le 'nous'. Par GABRIEL MADINIER. Paris: Félix Alcan. 1938. Pp. 143. 25 fr.

This is a philosophical essay. In a critical sense it is directed against objectifying and analysing tendencies which may lose sight of reality in a mass of dividing and individualizing distinctions. In a constructive sense it is an argument for an active attitude in which the 'I' is both realized and transcended in the 'we'. For this active attitude in its widest expression, the author finds the best name to be love.

Montaigne et l'Amitié. Par MAURICE RIVELINE. Paris: Félix Alcan. 1939. Pp. 268. 40 fr.

For the psychologically-minded reader the interest of this book lies chiefly in its excellent and appreciative study of the long and deep friendship between Montaigne and La Boétie. The essay falls into three parts, of which the first deals with friendship in the life of Montaigne, the second with the notion of friendship as held and developed by Montaigne, and the third with the influence of friendship in a general sense on the Essays. The whole is not merely an attractive excursion into literary criticism, but a significant study of one of the most important and influential of the human sentiments.

Essai sur les Origines Intuitives du Positivisme. Par PIERRE DUCASSÉ. Paris: Félix Alcan. 1939. Pp. 272+4 plates.

To many people positivism appeared, and, in so far as it is still influential, continues to appear, as a way of escape from a subjective and meditative way of thinking to something more externally directed and more in touch with the actual movement of life. At the same time it seemed to many to be rather a 'knack' of a systematic way of thinking, sweeping, regular, uniform, and not over-accommodating to the constant change of affairs. So Renan, for instance, made his famous prophecy, quoted near the beginning of this book: "I've come to believe that M. Comte will be 'une etiquette' in the future. He will have an important place in the future histories of philosophy. This

will be a mistake, I'm sure, but the future will make lots of other mistakes!" What M. Ducassé is largely concerned to show, is that positivism cannot be understood simply as an attempt to build a scientific philosophy. Its basis lies in Comte's own intuitive and affective life. Hence this essay is to a degree an intellectual biography of a philosopher. It is an extremely readable and lively essay, though to the English reader of the present day it may well seem a little on the long side.

The Boundaries of Science. By JOHN MACMURRAY. London: Faber and Faber, Ltd. 1939. Pp. 268. 7*s.* 6*d.* net.

A Bibliography of Psychological and Experimental Aesthetics, 1864–1937. By ALBERT R. CHANDLER and EDWARD N. BARNHART. University of California Press (England: Cambridge University Press). 1938. Pp. 189. 9*s.* net.

The Group Mind. By W. MCDOUGALL. Cambridge University Press. 1939. Pp. 304. 7*s.* 6*d.* net. A cheap edition of McDougall's well-known treatise.

Défense de la Vie. Par LOUIS VIALLE. Paris: Félix Alcan. 1938. Pp. 166. 16 fr.

La Conduite Humaine et les Valeurs Idéales. Par DOMINIQUE PARODI. Paris: Félix Alcan. 1939. Pp. 138. 15 fr.

Avoir une Âme. Par ÉTIENNE SOURIAU. Paris: Société d'édition *les belles lettres*. 1938. Pp. 141.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 30 SEPTEMBER 1939

Revenue Account for the year ended 30 September 1939

1938			Expenditure						Income					
£	s.	d.		£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.
170	0	0	*To* Rent, 55 Russell Square				180	0	0	*By* Subscriptions 1939			1475	0	6
252	0	0	Secretary's Salary				252	0	0	Subscriptions 1938			4	5	6
			Part-time assistance in Office ...				16	0	9				1479	6	0
93	5	0	Printing and Duplicating				109	16	7	*Less* Resignations and Removals ...			107	8	6
25	15	6	Expenses of Meetings				25	11	0						
			Office and General Expenses:							1325	10	6			1371	17	6
	£33	6	5 Caretaker, Lighting and Heating	33	11	8				123	17	8	Interest on Investments			127	2	4
	15	12	1 Stationery	18	6	0												
	68	0	9 Postages and Telephone ...	79	8	8												
	4	12	0 Fire, etc. Insurance	4	12	0												
			Subscriptions to outside Associa-															
	4	14	6 tions	4	14	6												
	10	10	0 Audit Fee 1938	10	10	0												
	7	13	11 *Repairs*															
	9	15	3 Teas	14	15	6												
	9	8	6 Sundry Expenses	5	11	5												
163	13	5					171	9	9									
			Journals Publication Expenses:															
	263	19	6 General	234	2	7												
	124	14	9 Medical	130	6	6												
	100	0	10 Educational	98	12	6												
488	15	1					463	1	7									
11	16	8	Extended General Meeting 1939 ...				9	10	7									
			Work for Committee on Human															
	19	1	Mental Measurements... ...				1	15	11									
4	4	0	Midland Branch Expenses... ...				2	11	8									
3	4	6	Northern Branch Expenses ...				1	5	0									
11	0	5	Scottish Branch Expenses... ...				8	12	5									
1	0	2	Cheque Books				1	0	0									
			Depreciation:															
			Library	61	17	9												
83	3	4	Office Furniture and Fittings ...	17	13	0	79	10	9									
1308	17	2					1322	6	0									
			Balance, being Surplus of Income															
140	11	0	over Expenditure				176	13	10									
£1449	8	2					£1498	19	10	£1449	8	2			£1498	19	10	

Balance Sheet as at 30 September 1939

1938	LIABILITIES					ASSETS					
£ s. d.		s.	d.	£	s. d.	£ s. d.		£	s.	d.	£ s. d.
2 2 6	Subscriptions paid in Advance ...			2	2 0	*210 2 1*	Cash at Barclays Bank Ltd.				274 15 7
						16 6	Postage Stamps in hand				1 19 1
	Publications Reserve:						*Investments at Cost:*				
122 0 0	Balance as per last Balance Sheet ...			122	0 0		£300 5% Conversion Loan 1944–64...	318	9	9	
	Revenue Account:						£3320. 14*s.* 8*d.* 3½% Inscribed War Loan	3425	7	9	
	Balance as per last Balance Sheet ...	4906	5 1			*3743 17 6*					3743 17 6
	Add Surplus of year	176	13 10				*Note:* Market value of above Securities at 30 September 1939, £3251				
4906 5 1				5082	18 11		Sundry Debtors				27 13 4
						129 15 0	Subscriptions in Arrear				183 0 0
							Office Furniture and Fittings:				
							Balance as per last Balance Sheet ...	172	3	0	
							Additions during year	3	10	0	
								175	13	0	
							Less Depreciation	17	13	0	
						172 3 0					158 0 0
							Library:				
							Balance as per last Balance Sheet ...	576	5	0	
							Add Expenditure during year ...	43	2	9	
								619	7	9	
							Less Depreciation	61	17	9	
						576 5 0					557 10 0
							Monograph Publications:				
							Balance as per last Balance Sheet ...	147	8	6	
							Add Grants made during year ...	90	0	0	
								237	8	6	
							Less Amounts repaid during year ...	27	3	1	
						147 8 6					210 5 5
						50 0 0	Shares in the British Journal of Educational Psychology Ltd., at Cost ...				50 0 0
£5030 7 7				£5207	0 11	*£5030 7 7*					£5207 0 11

I beg to report that I have examined the foregoing Accounts with the Books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shewn by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Bankers' Certificate.

T. BARRETT,
Incorporated Accountant

S. J. F. PHILPOTT,
Hon. Treasurer

31 *October* 1939.